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ORIGIN AND CLASSIFICATION OF CHEMICAL SEDIMENTS IN TERMS OF pH AND OXIDATION-REDUCTION POTENTIALS . . . W. C. KRUMBEIN AND R. M. GARRELS 1

EVALUATION OF CRITERIA FOR THE FORCIBLE INTRUSION OF MAGMA
JAMES A. NOBLE 34

SUBMARINE CANYONS BORDERING CENTRAL AND SOUTHERN CALIFORNIA
JOHN C. CROWELL 58

COMPOSITE ORIGIN OF SUBMARINE CANYONS . . . . . FRANCIS P. SHEPARD 84

GEOLOGICAL NOTES
NOTE ON MISSISSIPPIAN AND PERMIAN REEF SUITES . . . . . JOHN W. WELLS 97

DISCUSSION
ORIGIN OF MICRORELIEF IN THE LAKE AGASSIZ BASIN . . . . . C. C. NIKIFOROFF 99

REVIEWS 104

ERRATUM 106

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ORIGIN AND CLASSIFICATION OF CHEMICAL SEDIMENTS IN TERMS OF pH AND OXIDATION-REDUCTION POTENTIALS[1]

W. C. KRUMBEIN AND R. M. GARRELS
Northwestern University

ABSTRACT

Chemical sediments of marine origin are divided into three main classes, representing deposition in normal marine open-circulation environments, restricted humid (euxinic) environments, and restricted arid (evaporite) environments. The characteristics of these environments are briefly reviewed, and the position is taken that the hydrogen-ion concentration, pH, and the oxidation-reduction potential, Eh, afford two basic controls which largely determine the kinds of chemical end-members produced by both inorganic and biochemical reactions.

Within the framework of these controls, the depositions of calcium carbonate, iron minerals, manganese minerals, phosphates, evaporites, and organic matter are shown in their relation to variations in pH and Eh of the environment. Certain of the end-members depend mainly upon one or the other of the two controls, and some depend upon both. A classification of chemical sediments is proposed which shows their relation to pH and Eh and serves to indicate genetic relations among the chemical end-members.

Occurrences of chemical end-members are summarized on a pH-Eh graph, which shows the range of these values which normally occurs in the environments discussed. Observed mineral associations in typical chemical sediments are listed in support of the theoretical treatment. The influence of postdepositional changes on the original mineral associations is also pointed out. The writers conclude that the environment of deposition of many ancient chemical sediments can be reconstructed in terms of its essential physicochemical characteristics from study of the mineral assemblages among the chemical end-members present in the sediment.

INTRODUCTION

Chemical sediments include, among others, carbonates (limestone and dolomite), sulfates (anhydrite and gypsum), chlorides (halite and sylvite), silica (chert), iron compounds (hematite and siderite), phosphates (phosphorite), manganese compounds (rhodochrosite), and organic deposits (coal and bitumen). The chemical sediments may occur as relatively pure substances, represented by calcitic limestone and anhydrite, or as mixtures with detrital material or with other chemically formed sediments.

From the viewpoint of sedimentary classification, the pure sediments may be treated as chemical end-members, described and discussed by Krynine (1948), Pettijohn (1949), and Krumbein and Sloss (1951). On this basis, triangular or tetrahedral groupings may be used in studying compositional relations among the chemical end-members or between chemical and detrital end-members. For present purposes, in which the chemical end-members are emphasized, the latter mixtures may be considered as representing an influx of chemically inert detrital material into the environment to "dilute" the chemical end-members.

[1] Manuscript received July 3, 1951.

Although virtually any chemical end-member may occur mixed in any proportion with detrital material, the occurrence of two or more chemical end-members in a single sediment is subject to certain limitations. Thus some chemical end-members, such as calcium carbonate, may occur in intimate association with almost any other chemical end-member, as in bituminous limestone, gypsiferous limestone, sideritic limestone, and so on. On the other hand, such an association as pyritiferous anhydrite is rarely or never observed.

Chemical sediments may form in a wide variety of environments, ranging from hypersaline solutions to fresh water and representing all gradations from normal open-circulation to stagnant conditions. Evaporites of marine origin, for example, have been related directly to precipitation from concentrated solutions. Evaporites thus represent development of chemical sediments from certain restrictions within the environment. Similarly, sediments with abundant organic matter and pyrite represent other kinds of environmental restrictions, commonly involving stagnation of the depositional medium. In contrast to these examples, some chemical sediments, such as lime mud, may be formed in well-oxygenated waters with open circulation.

Because of the wide range of conditions under which chemical sediments are formed, it has been common practice to discuss each type as a separate class rather than to formulate classifications which take account of interrelations among them. Description and classification of the chemical sediments are based primarily on composition, with texture as a minor element, although numerous textural groups of limestone are recognized. Complete genetic classifications of the chemical sediments are limited in part by incomplete knowledge of the environmental conditions required for their formation and in part by the complexity of the physicochemical and biochemical reactions involved in their deposition.

Despite difficulties in establishing relations among the chemical sediments, a large background of knowledge is available on the mineralogical and lithological associations of the sediments, on some of their environmental implications, and on fields of chemical stability of the end-members. Any proposed classification or interpretation of the chemical sediments must accordingly satisfy this background of accumulated knowledge. It is the purpose of this paper to review some aspects of this common knowledge and to propose a basis for classification of chemical sediments which takes account of observed relations among the chemical end-members.

ENVIRONMENTS OF CHEMICAL SEDIMENTATION

In order to set a framework for discussion, this paper is limited mainly to chemical sediments of marine origin. Within this framework two main kinds of environments are involved. The first is the normal marine open-circulation environment as represented by the open sea. The second broad environment involves restricted conditions, in which physiographic, tectonic, or biologic features impose controls on circulation, oxygenation, or concentration of dissolved salts. The restricted environment itself is divisible into restricted humid environments, responsible for euxinic deposits, and restricted arid environments, responsible for evaporites.

NORMAL MARINE OPEN-CIRCULATION ENVIRONMENT

The characteristics of the open-sea environment are described and discussed in detail by Fleming and Revelle (1939), by Sverdrup, Johnson, and Fleming (1942), and by Kuenen (1950). The several topics reviewed here are taken mainly from these sources and emphasize portions which bear on the formation of chemical sediments.

Some of the characteristics of the normal marine open-circulation environment are summarized in table 1. As may be seen, the temperature varies more widely near the surface than at depth and, in any event, is confined to a relatively small range. The salinity is nearly uniform geographically and with depth. About forty-four elements have been detected in sea water, but some ten of these are quantitatively the most impor-

TABLE 1

CHARACTERISTICS OF THE NORMAL MARINE OPEN-CIRCULATION ENVIRONMENT

Item	Average Value	Remarks
Temperature	Surface: 0°–25° C.	Varies with latitude, depth, currents
Pressure	Surface: 1 atm. Bottom: see remarks	Pressure increases 1 atm. for each 30 feet of depth Maximum pressure in ocean bottom is about 1,000 atm.
Salinity	35 parts per thousand	Varies from 33 to 37 parts per thousand as function of latitude; highest in tropics
Ions in solution	*Constants* (*parts per thousand*): Cl^- 19.34 $SO_4^=$ 2.70 Na^+ 10.72 Mg^{++} 1.30 Ca^{++} 0.42 K^+ 0.38	Not appreciably affected by organisms; variations introduced near river mouths, etc.
	Variables (*parts per million*): Phosphorus 0.001–0.1 Iron 0.002–0.02 Silicon 0.02 –4.0 Manganese 0.001–0.01	Strongly affected by biologic activity
Dissolved gases	*Oxygen:* Surface: 6 cc/liter Bottom: 1 cc/liter at depth of 2,000 ft.	Varies from about 4.9 to 9.0 cc/liter
	Carbon dioxide: Surface: 46 cc/liter Bottom: variable (usually complement of O_2)	Surface water in equilibrium with atmosphere; amount at depth a function of photosynthesis, etc.
	Hydrogen sulfide: Surface: nil Bottom: nil	Usually not present in open-circulation conditions
Hydrogen-ion concentration (pH)	Surface: 8.2 Bottom: 7.8	Ranges from 7.5 to 8.4
Oxidation-reduction potential (Eh)	Surface: 0.2–0.4 Bottom: 0.1–0.3	Sea water is mildly oxidizing throughout

tant. The solids dissolved in sea water fall into two groups. Iron, silica, manganese, phosphate, and nitrogen compounds are variable in amount, inasmuch as they are nutrients, whereas other ions, such as sodium, magnesium, chloride, and sulfate, are not concentrated by organisms and hence remain fairly uniform in amount.

The gases dissolved in sea water also show a range from relative constancy to a high degree of variability. Surface waters are essentially "saturated" with oxygen, carbon dioxide, and nitrogen; they are in equilibrium with the atmosphere. Nitrogen presumably remains uniform, whereas both oxygen and carbon dioxide vary because of organic processes.

The chemical state of the water—i.e., its acidity or alkalinity and its reducing or oxidizing nature—is also fairly uniform for the open sea. The hydrogen-ion concentration, pH, varies from 7.8 to 8.4, which represents mildly alkaline conditions. The oxidation-reduction potential, Eh, has small positive values, representing mildly oxidizing conditions.[2]

Fleming and Revelle (1939) emphasize that the ocean is a single entity in which all the parts are interrelated and interdependent. No solid boundaries separate one part from another, and the possibility of complete uniformity in physical, chemical, and biological conditions is always present. One consequence of the uniformity of the ocean is that events occurring in any part of the sea will be effective over great distances. Hence the normal marine open-circulation environment is characteristically a domain of widespread occurrence of similar chemical sediments conditioned locally by relatively small variations in the controlling conditions.

The characteristics of the open sea as listed in table 1 generally prevail to the depositional interface, but below that surface the changes in physicochemical conditions may be pronounced.

Prominent changes below the depositional interface may involve depletion of oxygen and decrease of Eh to negative values. The pH remains about 8, so that normal alkaline conditions still prevail. The change from alkaline oxidizing to alkaline reducing conditions may strongly influence the stability of chemical sediments formed in the water above the interface. However, the negative Eh represents a potential and indicates a tendency toward reduction. Actual reduction depends upon the reducing capacity of the environment, so that whether or not a reducing reaction takes place is more a function of the capacity than of the potential. The reducing capacity diminishes with depth below the interface, and it may be expected that chemical end-members which pass through the upper part of the diagenetic environment without change are less likely to be reduced with additional burial.

Sand bottoms in well-oxygenated waters have a positive Eh below the depositional interface (ZoBell, 1946), so that chemical end-members which are stable in the oxidizing environment above the interface are presumably not subject to severe chemical change as they are buried in such environments. The influence of postdepositional changes on chemical end-members in specific cases will be outlined later in this paper.

RESTRICTED HUMID (EUXINIC) ENVIRONMENT

Restrictions on circulation, imposed by a variety of controls, may change or

[2] The reader is referred to ZoBell (1946) for detailed definitions and methods of measuring Eh and pH.

impede currents, change the content and nature of dissolved salts and gases, and, in general, produce marked changes in the pH and Eh of the resulting waters. In this review of restricted environments, the simplest case of restriction by submerged sills, as treated by Fleming and Revelle (1939), Ström (1939), Sverdrup, Johnson, and Fleming (1944), and Kuenen (1950) will be considered.

If the sill which impedes circulation is narrow and deep, surface waters may flow across the sill in one direction while deeper waters flow in the opposite direction, as occurs at the western entrance to the Mediterranean. If the sill is broad and shallow, flow may be predominantly in one direction. This latter situation provides the clearer case and will be illustrated here, inasmuch as it applies equally well to restricted humid and restricted arid conditions.

The restricted humid environment involves at least partial stagnation in the basin enclosed by the sill, with normally oxygenated waters at the surface. In humid climates where stream flow exceeds evaporation, the surface waters may become diluted by stream inflow, and the excess water flows seaward over the submerged sill without affecting the water behind the barrier. Table 2 lists

TABLE 2

CHARACTERISTICS OF THE RESTRICTED HUMID (EUXINIC) ENVIRONMENT

Item	Average Value	Remarks
Temperature	Surface: 16° C. Bottom: 6° C.	Values apply to Norwegian fiords: higher values in tropical basins
Pressure	See table 1	
Salinity	Surface: 19 parts per thousand Bottom: 30 parts per thousand	Probably a wide variation from near-fresh waters at surface to normal salinity at depth
Ions in solution	*Constants:* Relative proportions of ions may remain the same as in table 1, except for $SO_4^=$ conversion to $S^=$ *Variables:* Phosphate ion usually high: 0.3 mg/liter in fiords	
Dissolved gases	*Oxygen:* Surface: 6 cc/liter Bottom: nil *Carbon dioxide:* Surface: 46 cc/liter Bottom: 46 + cc/liter *Hydrogen sulfide:* Surface: nil Bottom: 9–14 cc/liter	Maximum observed in fiords 40 cc/liter
Hydrogen-ion concentration (pH)	Surface: 8.0 Bottom: 7.0	Some fiords show pH less than 7.0 near bottom, indicating definitely acid conditions
Oxidation-reduction potential (Eh)	Surface: 0.1 Bottom: −0.3	

some of the characteristics of stagnant humid basins, summarized mainly from Fleming and Revelle (1939) and Ström (1939). The data pertain mainly to Norwegian fiords, and the average values given by the present authors are probably only approximate. Nevertheless, comparison of table 2 with table 1 shows several striking contrasts. Instead of a fairly uniform water body like the open sea, stagnant basins show marked differences between surface water and water at depth. The most striking of these changes in fiords is the depletion of oxygen at intermediate depths and the increasing concentration of dissolved hydrogen sulfide toward the bottom. Another marked change is from lesser salinity at the surface to higher salinity at depth. Not only does concentration of dissolved ions change with depth, but the nature of the ions may change significantly. Thus concentration of the phosphate ion, very small or negligible at the surface, may be relatively high near the bottom.

The chemical state of the water, as represented by pH and Eh, is also strikingly different from open sea water. At the surface the Eh has small positive values, which decrease to zero at about the point where oxygen is depleted. Below this depth, the Eh becomes negative and may attain values of -0.3 at the bottom. The pH, essentially normal at the surface, decreases to about 7.0 at the bottom, and in some fiords values less than 7.0 have been observed, indicating definitely acid conditions.

RESTRICTED ARID (EVAPORITE) ENVIRONMENT

The restricted arid environment occurs in regions where evaporation greatly exceeds precipitation and stream inflow. Present examples include the Red Sea, the Mediterranean Sea, the inner part of the Gulf of California, and the Gulf of Karabugas. The latter is the classic evaporite environment.

Sverdrup, Johnson, and Fleming (1942) point out that evaporation is at a maximum in winter, during which the surface density is sufficiently increased to develop vertical convection currents. These currents renew the oxygen in the bottom waters and establish a density in the basin greater than that in the normal open-circulation sea water beyond the confining sill. Where the entrance over the sill is sufficiently deep, the higher-density water flows outward from the basin, and less dense sea water flows inward at the surface. These conditions exist in the first three basins mentioned above.

The Gulf of Karabugas represents an extreme case in which the restricting sill is so shallow that outflow of dense water does not occur. Instead, a unidirectional current flows from the Caspian Sea into the gulf. As a result, the salinity in the gulf is very high, up to 164 parts per thousand in 1902.

The writers were not able to find specific data on the arid environment for most of the items listed in tables 1 and 2, especially on such features as the pH and Eh of sea water concentrated by evaporation. If it is generally true that the bottom waters are renewed by vertical currents, it would appear that the Eh of the water should have small positive values. Ström (1939) cites examples of saline lakes with foul bottom waters and black muds beneath the bottom crust. The pH of the solution may very likely change as evaporation proceeds, especially after some salts have been precipitated. In general, it is believed that the concentrated solutions would be more alkaline than the original sea water. The presence of ferric rather than ferrous

compounds in ancient evaporite associations supports the inference of positive Eh values, and the general prevalence of normal salts in the evaporites implies neutral to mildly alkaline conditions.

Temperatures in evaporating basins probably have higher values, on the average, than in the normal marine open-sea environment. The presence of primary anhydrite in many ancient evaporites implies temperatures of the order of 30° C. or higher. On the other hand, primary gypsum also occurs, suggesting that some evaporation occurred at more normal temperatures.

Table 3 is an attempt to summarize observed and inferred conditions in a restricted arid environment which is depositing salt (halite). The values shown for the several items are of value mainly in contrasting the evaporative conditions with those in the normal open-sea environment and with the restricted humid environment.

CONTROL OF SEDIMENTATION BY Eh, pH, AND ACTIVITY PRODUCTS

Precipitation of chemical sediments in the open sea or under restricted conditions may result from organic or inorganic processes. It is likely that precipitation through organic agencies (shell-forming organisms, bacteria, algae, etc.) is more important than purely inorganic

TABLE 3

INFERRED CHARACTERISTICS OF THE RESTRICTED ARID (EVAPORITE) ENVIRONMENT

Item	Average Value	Remarks
Temperature	30° ± C.	Presence of anhydrite and other temperature-dependent salts suggests values may exceed 40° C.
Pressure	See table 1	Density increase to 1.2 slightly increases pressure effect
Salinity	325 parts per thousand ±	Halite begins to deposit when sea-water volume is reduced to approximately one-tenth
Ions in solution (parts per thousand)	Na^+ 100 ± K^+ 4 Mg^{++} 12 Ca^{++} 1 $SO_4^=$ 15 Cl^- 180 Br^- 1	Approximate values based on Usiglio's data (Clarke, 1924, p. 220)
Dissolved gases	Oxygen: 1–2 cc/liter Carbon dioxide: 5–10 cc/liter Hydrogen sulfide: nil	Values estimated from salt and temperature effects on gas solubility Possible exceptions brought out later in text
Hydrogen-ion concentration (pH)	8–9	Limited data indicate pH increases with increasing salinity; solution probably poorly buffered
Oxidation-reduction potential (Eh)	0.0 to +0.1	High temperature reduces oxygen content and presumably may decrease Eh

processes in the normal marine open-circulation environment. On the other hand, the very high salinity in evaporite basins inhibits organisms, and inorganic precipitation from saturated solutions appears to be the rule.

The wide variety of inorganic chemical processes and biochemical reactions which may form chemical sediments renders it difficult to segregate the sediments on the basis of one or another process. Calcium carbonate, for example, may be produced in a variety of ways, but a definitive identification of the specific agency which formed a given ancient limestone may not generally be possible. It seems more logical to the writers, therefore, to select those broad controls of chemical precipitation which include biochemical processes as well as purely inorganic reactions. In this way a description of each reaction is not necessary, and a framework broad enough for all chemical sediments can be established.

The writers believe that the pH and Eh of the environment provide general controls on chemical sedimentation which include both inorganic reactions and biochemical processes. It can be shown that the chemical end-members are dependent upon one or both of these controls, whether the reactions which form them are purely inorganic or include the activity of organisms. Review of the conditions controlling calcium carbonate precipitation illustrates the line of reasoning followed.

PRECIPITATION OF CALCIUM CARBONATE

No chemical or organochemical end-member forms in so many diverse environments and has such a wide variety of associated minerals as calcium carbonate. One clue to its ubiquitous occurrence is the lack of dependence of both calcium and carbonate ions on the Eh of the solution. Oxidation-reduction potentials sufficient to reduce or oxidize either of these ions to a higher or lower state do not occur in the sea; hence stagnation, with its resultant very low Eh values, is not a hindrance to calcite formation, nor is strong oxygenation. On the other hand, the carbonate ion is strongly dependent on pH, and its repression into bicarbonate ion and carbonic acid at low pH values accounts for the high solubility of calcite in acid solutions. In very broad terms, increase in pH tends to cause precipitation of calcite, and environments of low pH are not likely to reach saturation.

Obviously, pH is not the only control of deposition, but it seems to be the most important. Other controls are either minor or act through a pH mechanism. For example, increase in temperature causes precipitation of calcite from a saturated solution of sea water, but the effect is largely because of an increase of pH through removal of CO_2 (cf. Emery, 1946). Changes in CO_2 pressure in the atmosphere act in the same way. Organisms act through processes that in many cases are extremely complex, but the viewpoint of the writers, as previously stated, is that no organic mechanisms can operate to produce calcium carbonate in an environment that is far out of equilibrium with inorganic calcium carbonate; therefore, pH still is a controlling factor. Solubility can be changed by changes in salinity at constant pH and also by pressure, but the effect of these variables is relatively slight compared to pH, even over extreme ranges. In summary, the independence of calcium and carbonate ions of oxidation-reduction potentials explains calcite occurrence over the entire range from well-oxygenated to stagnant-water environments. The

pH dependence of carbonate ion suggests that most precipitation of calcium carbonate takes place because of an increase in pH, whether the mechanism be organic or inorganic, and that truly acid environments (pH 7.0 or less) are unfavorable to limestone deposition. This statement does not imply that most limestones are chemical deposits.

CONTROLS OF PHOSPHATE DEPOSITION

The controlling factors in the deposition of phosphorite (essentially tricalcium phosphate) are strikingly similar to those of calcium carbonate. Neither calcium nor phosphate ion is affected by Eh changes within limits which occur in nature; phosphate ion, on the other hand, is strongly pH-dependent. Consequently, phosphorite solubility, in terms of sea water, decreases with increasing pH, and its solubility curve is essentially parallel to that of calcium carbonate, although the absolute values are much less. According to Dietz, Emery, and Shepard (1942), the open ocean is essentially saturated with phosphate, indicating that an amount equal to the increment is precipitated each year.

When calcite and phosphorite co-precipitate because of pH increase, the ratio is very high in favor of calcite. Both carbonate ion and phosphate ion are in equilibrium with the same calcium ion in sea water. Because the absolute solubility of calcium phosphate is so much less than that of calcite and because the solubility-pH curves are nearly parallel, it follows that precipitation by pH increase of sea water saturated with both compounds will result in a sediment composed almost entirely of calcite and with only a trace of phosphorite.

A sediment high in calcium phosphate and low in calcium carbonate can be formed only where conditions permit continuous removal of calcium as the phosphate and in which the activity product of the carbonate is not exceeded. This might well occur in a restricted basin with a relatively low pH (7.0–7.5). In this environment calcium carbonate would not be expected to form. The carbonate ion remains low and constant because of its dependence on pH and the CO_2 partial pressure in the atmosphere. On the other hand, the phosphate ion might well be supplied as "soluble phosphate" in association with sodium or other cation, and the phosphate-ion concentration could easily become sufficient to precipitate phosphorite, for it has no upper limit determined by formation of a gaseous phase, as does carbonate.

In summary, the phosphate ion is independent of Eh, but increases with increasing pH. Precipitation of phosphorite from normal sea water (saturated with both calcite and phosphorite) as a result of pH increase would be expected to yield a sediment with a very small proportion of phosphorite. It is suggested that the phosphorites may represent precipitation in restricted basins with relatively low pH.

THE HEMATITE-SIDERITE-PYRITE SYSTEM

The foregoing discussion of calcium carbonate and calcium phosphate controls has been entirely qualitative. It is possible, however, to treat systems of related groups of minerals at least semiquantitatively and to prepare diagrams showing their fields of chemical stability as functions of pH and oxidation-reduction potentials. The iron mineral group hematite, siderite, and pyrite has been chosen as an example to show in detail the way in which such diagrams can be constructed.

In a marine environment of constant

salinity and temperature, precipitation of these minerals depends upon the activity of ferrous and ferric ions, carbonate ion, sulfide ion, and hydroxyl ion. If reasonably accurate values of these activities and the activity products for the minerals at the given temperature are known, then Eh-pH fields of stability for all species can be calculated. The activity products at 25° C. are (Latimer, 1940, pp. 309–317):

Hematite: $(a_{Fe^{+++}})(a_{OH^-})^3 = 4 \times 10^{-38}$,

Siderite: $(a_{Fe^{++}})(a_{CO_3^=}) = 2.1 \times 10^{-11}$,

Pyrite:[3] $(a_{Fe^{++}})(a_{S^=}) = 1 \times 10^{-19}$.

Very broadly, then, the activity product of hematite is most likely to be exceeded in solutions of high Eh and high pH, for they would have a high Fe^{+++}/Fe^{++} ratio and a high hydroxide concentration. Siderite would be favored by solutions of low Eh and high pH, providing a high Fe^{++}/Fe^{+++} ratio and high carbonate-ion concentration. Pyrite would be favored by solutions of low Eh and high pH also, for they promote the formation of both Fe^{++} ion and $S^=$ ion. If pairs of values of Eh and pH are chosen, it is possible to calculate which of the three minerals will precipitate, and thus to delimit stability fields as a function of Eh and pH. These fields are essentially independent of concentration.

1. *The ferric-ferrous ratio.*—The ferric-ferrous ratio is controlled by Eh alone and is determined by the equation (Castano and Garrels, 1950):

$$\mathrm{Eh} = E_o + 0.06 \log \frac{a_{Fe^{+++}}}{a_{Fe^{++}}},$$

where Eh is the oxidation potential of the system (sea water) and E_o is the standard electrode potential for the ferrous-ferric half-cell. The term 0.06 log is the numerical value at 25° C. of the more general term (RT/nF) ln. The value of E_o is 0.77 volts at 25° (Latimer, 1938, p. 296); consequently, the ratio $a_{Fe^{+++}}/a_{Fe^{++}}$ can be calculated for any given Eh, or, conversely, Eh can be determined for any ferric-ferrous ratio.

2. *Carbonate-ion concentration.*—The major controls of carbonate-ion concentration were discussed in a preceding section. Values for carbonate-ion concentration in sea water can be read directly from a graph in Sverdrup (1942, p. 201). The major controls are salinity, total CO_2 content (ΣCO_2), and pH. The values used in the calculations in this section are for normal sea water. Abnormal salinity or partial pressure of CO_2 in the atmosphere would change the values, but the effect on the stability fields, as will be shown later, is not great.

3. *Sulfide-ion concentration.*—Sulfide-ion concentration depends upon the total sulfide plus sulfate in the sea water and the $SO_4^=/S^=$ ratio. The value of total sulfide plus sulfate used here is that of normal sea water. Again it will be demonstrated later that the stability-field boundaries are not particularly sensitive even to large deviations from this value.

The sulfide-sulfate ratio depends on both Eh and pH. Ridge (1948) discussed several equations involving the oxidation of sulfide ion to sulfate. Adding his equations together stepwise, one obtains

$$S^= + 4H_2O = SO_4^= + 8H^+ + 8e;$$

$$E_o = +0.14 \text{ volts}.$$

In terms of Eh:

$$\mathrm{Eh} = E_o + \frac{RT}{nF} \ln \frac{(a_{SO_4^=} a_{H^+}^8)}{(a_{S^=})}.$$

Assigning numerical values to E_o and to

[3] Pyrite, of course, is FeS_2, but it is certainly less soluble than FeS, so the known activity product of FeS represents a maximum value for true pyrite.

RT/nF and changing to logarithms of base 10, we obtain

$$\mathrm{Eh} = 0.14 + 0.0075 \log \frac{a_{SO_4^=} a_{H^+}^8}{a_{S^=}},$$

and the ratio $a_{SO_4^=}/a_{S^=}$ can be calculated for any given values of Eh and pH.

4. *Determination of stability fields.*—With a knowledge of the $a_{Fe^{+++}}/a_{Fe^{++}}$ ratio, $CO_3^=$-ion concentration, and $S^=$-ion concentration as functions of Eh and/or pH, we can return to the activity products of hematite, siderite, and pyrite, and delimit their stability fields.

a) *Hematite-siderite.*—Consider, first, the hematite-siderite relation:

$$(a_{Fe^{+++}})(a_{OH^-})^3 = 4 \times 10^{-38},$$

$$(a_{Fe^{++}})(a_{CO_3^=}) = 2.1 \times 10^{-11}.$$

At any assumed pH, $a_{CO_3^=}$ and a_{OH^-} are fixed. For example, at a pH of 8, a_{OH^-} is 10^{-6}, and $a_{CO_3^=}$ (Sverdrup, Johnson, and Fleming, 1942, p. 201) is 4.4×10^{-6}. Substituting these values in the equations for activity products, we obtain

$$a_{Fe^{+++}}(10^{-6})^3 = 4 \times 10^{-38},$$

$$a_{Fe^{++}}(4.4 \times 10^{-6}) = 2.1 \times 10^{-11}.$$

At the boundary between the hematite and siderite stability fields, both compounds are in equilibrium—that is, their solubility is the same, and both equations are satisfied. Dividing the first by the second, we have

$$\frac{a_{Fe^{+++}}}{a_{Fe^{++}}} = \frac{4 \times 10^{-20}}{4.8 \times 10^{-6}} = 8.3 \times 10^{-15}.$$

At this value of the ratio, the Eh can be calculated from

$$\mathrm{Eh} = E_o + 0.06 \log \frac{a_{Fe^{+++}}}{a_{Fe^{++}}}.$$

Substituting, we obtain

$$\mathrm{Eh} = 0.77 + 0.06 \log 8.3 \times 10^{-15},$$

$$\mathrm{Eh} = -0.08.$$

From a series of such calculations of Eh at arbitrarily selected pH values, the Eh-pH boundary between siderite and hematite can be determined. The calculated values are given in table 4.

TABLE 4

EH AND PH VALUES AT WHICH HEMATITE AND SIDERITE ARE IN EQUILIBRIUM WITH SEA WATER AT 25° C.

pH	Eh	$a_{Fe^{+++}}$	$a_{Fe^{++}}$
6	+0.16	4×10^{-14}	7×10^{-4}
7	+ .03	4×10^{-17}	8.8×10^{-5}
8	− .08	4×10^{-20}	4.8×10^{-6}
9	−0.22	4×10^{-23}	1.5×10^{-6}

b) *Siderite-pyrite.*—The next step is to show that the activity product of pyrite is not exceeded at the hematite-siderite boundary, and then to calculate the siderite-pyrite boundary.

At a pH of 8 and an Eh of −0.08, the activity product of siderite is reached at an activity of ferrous ion of 4.8×10^{-6} mols per liter. If these values of Eh and pH are substituted in the equation for determining the $SO_4^=/S^=$ ratio, we have

$$\mathrm{Eh} = +0.14 + 0.0075 \log \frac{a_{SO_4^=} a_{H^+}^8}{a_{S^=}}, \quad (1)$$

$$-0.08 = 0.14 + 0.0075 \log \frac{a_{SO_4^=} 10^{-64}}{a_{S^=}},$$

$$\log \frac{a_{SO_4^=}}{a_{S^=}} = 34.7,$$

$$\frac{a_{SO_4^=}}{a_{S^=}} = 10^{34.7}.$$

Consequently, if total sulfate plus sulfide (Sverdrup, Johnson, and Fleming, 1942, p. 166) is approximately 3×10^{-2} mols per liter, then the activity of the sulfide ion is roughly

$$a_{S^=} = 6 \times 10^{-37}.$$

Thus the product of ferrous ion and

sulfide ion is

$$4.8 \times 10^{-6} \times 6 \times 10^{-37} = 2.9 \times 10^{-42},$$

and the activity product of pyrite (10^{-19}) is far from being exceeded.

The siderite-pyrite boundary lies at those values of Eh and pH where the respective activity products are simultaneously equaled, thus,

$$a_{Fe^{++}} a_{CO_3^=} = 2.1 \times 10^{-11}$$

$$a_{Fe^{++}} a_{S^=} = 1 \times 10^{-19}.$$

Dividing, we have

$$\frac{a_{CO_3^=}}{a_{S^=}} = 2.1 \times 10^{8}.$$

At a given pH, which defines $a_{CO_3^=}$, it is possible to calculate the Eh to provide sufficient sulfide ion for the formation of pyrite.

At a pH of 8, $a_{CO_3^=}$ is 4.4×10^{-6}; then the activity of sulfide necessary for pyrite formation is

$$a_{S^=} = \frac{4.4 \times 10^{-6}}{2.1 \times 10^{8}} = 2.1 \times 10^{-14}.$$

Because total sulfide plus sulfate is 3×10^{-2}, then $a_{S^=}$ will be 2.1×10^{-14} when:

$$\frac{a_{SO_4^=}}{a_{S^=}} = \frac{(3 \times 10^{-2}) - (2.1 \times 10^{-14})}{(2.1 \times 10^{-14})},$$

$$\frac{a_{SO_4^=}}{a_{S^=}} = 1.4 \times 10^{12}.$$

Substituting in equation (1), we obtain

$$\text{Eh} = 0.14 + 0.0075 \log (1.4 \times 10^{12}) \times (10^{-64}),$$

$$\text{Eh} = -0.22.$$

Similar calculations at other pH values yield the data given in table 5.

The values of this table and the preceding one can be combined to give an Eh-pH diagram showing the stability fields of hematite, pyrite, and siderite, as shown in figure 1.

5. *Effect of other variables on stability boundaries.*—Sufficient data are not available for a complete analysis of the effect of temperature, pressure, and composition on the boundaries of the fields of the iron minerals, but most of the effects can be evaluated.

TABLE 5

Eh AND pH VALUES AT WHICH PYRITE AND SIDERITE ARE IN EQUILIBRIUM WITH SEA WATER AT 25° C.

pH	Eh	$a_{CO_3^=}$	$a_{S^=}$
6	−0.11	3×10^{-8}	1.4×10^{-16}
7	− .18	2.4×10^{-7}	1.1×10^{-15}
8	− .25	4.4×10^{-6}	2.1×10^{-14}
9	−0.31	1.4×10^{-5}	7×10^{-14}

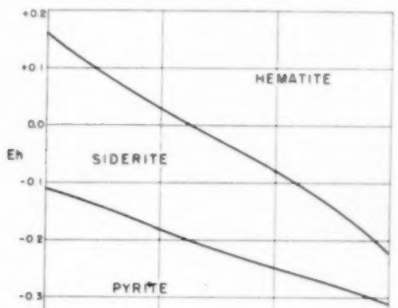

FIG. 1.—Fields of stability of hematite, siderite, and pyrite.

a) Effect of pressure.—The effect of hydrostatic pressure on the field boundaries is very small. Pressure change from 1 to 1,000 bars would be expected to increase the equilibrium constants not more than about eightfold (Owen and Brinkley, 1941, p. 472). Furthermore, the increase in the various equilibrium constants and activity products is not in the same direction. The effect of pressure in-

crease on the solubility of siderite, for example, is to increase solubility by virtue of an increase in the activity product of siderite and to decrease the solubility because of release of carbonate ions from bicarbonate ions. Consequently, since pressure effects are measured in terms of solubility changes less than tenfold, whereas the effect of a change of 0.1 of an Eh unit is of the order of 10^{10} fold, pressure does not seem to be an important variable.

b) Effect of temperature.—The range of temperature to be expected in normal marine open-circulation environments is not very great—roughly from 0° to 25° C. Temperature coefficients of all the equilibria involved are not known, but, for those that are available, the effect of such a temperature range is comparable to an increase of 1,000 bars in pressure, not more than a tenfold increase of the equilibrium constants. Increase in temperature essentially causes an increase in activity products or equilibrium constants, but the order of magnitude is not changed for those species for which values are known. Such effects are unimportant relative to those of Eh and pH. For example, solubility of hematite is changed by a factor of 10^3 by a change of 1 pH unit.

c) Effect of composition.—It is perhaps surprising that the field boundaries for iron compounds are essentially independent of the concentration of the components. The relation between hematite and siderite, for example, depends upon ferrous ion activity, ferric ion activity, carbonate-ion activity, and hydroxyl. The activity of hydroxyl ion is, of course, determined entirely by pH. The activity of carbonate ion is a function of pH, salinity, and total CO_2 in solution. It is not particularly sensitive to salinity, and CO_2 content in the atmosphere is reasonably constant. The ferrous-ferric ratio is constant at a given Eh; increase in total iron would increase the amount of siderite or hematite precipitated but would not cause a change from siderite to hematite.

6. *Summary.*—It appears that the boundaries between the fields of stability between pyrite, siderite, and hematite as delimited on an Eh-pH diagram are essentially independent of temperature, pressure, and composition of the seawater system from which they precipitate and that the maximum effects to be expected from the secondary variables probably can be accounted for by drawing a narrow zone of transition at the stability-field boundaries. It should be emphasized, perhaps, that the amount of a particular mineral that will precipitate depends upon the amount of the constituents available but that a change in deposition from one mineral to another will not take place unless there is a change in the Eh or pH of the environment.

THE MANGANESE OXIDES: RHODOCHROSITE-ALABANDITE SYSTEM

Manganese minerals form a very close parallel to the iron minerals. The activity products of the carbonate and of the sulfide are of the same order of magnitude as those for corresponding iron compounds. Unfortunately, few data are available on the activity products of the many naturally occurring manganese oxides. An Eh-pH diagram would be expected to be similar to that for the iron minerals. Figure 2 shows the inferred relations of the manganese minerals. The exact position of the boundary lines between fields is open to considerable question, but, whatever the true values may be, the boundary lines would be parallel or nearly parallel to the ones

shown. The boundary between the manganese sulfide and carbonate presumably is considerably lower than that for the corresponding iron species, as evidenced by the lack of information on the occurrence of the manganese sulfide in sediments.

IRON MINERALS: LIMESTONE SYSTEM

A knowledge of the fields of stability of the iron minerals is not enough in itself to answer the question of the relative amounts of iron minerals that will precipitate for a given change of environmental conditions. To answer that question, it is necessary to know actual activities. Accordingly, the activity of the iron minerals has been plotted in figure 3 in contour form on an Eh-pH diagram. The stability fields also are indicated. In addition to these relationships, the activity of calcium ion has been contoured on the surface. Because both carbonate ion and calcium ion are Eh-independent, the contours for calcite solubility[4] run parallel to the Eh axis. Carbonate ion, however, is strongly pH-dependent. As a matter of fact, the calcite solubility surface is parallel to the siderite solubility surface, although the order of magnitude of the solubility is approximately two hundred times as great.

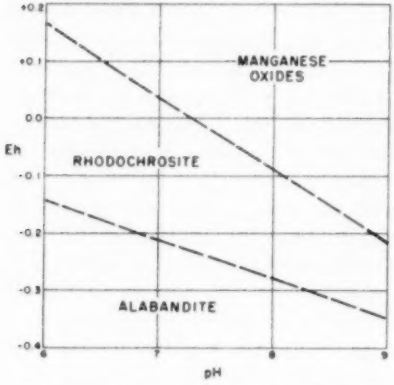

FIG. 2.—Approximate fields of stability of manganese oxides, rhodochrosite, and alabandite.

From figure 3 it is possible to examine the conditions for the precipitation of any of the iron minerals alone or for the precipitation of iron minerals together with limestone. For example, a solution saturated in iron and calcium at point A would contain approximately 10^{-1} mols per liter calcium and 10^{-3} mols per liter iron. If some of this solution were moved directly from A to B, both siderite and calcite would precipitate; and at point B the solution would contain approximately 10^{-2} mols per liter of calcium and 10^{-4} mols per liter of iron. In other words, such a change in conditions would result in the precipitation of both siderite and calcite in a constant ratio of approximately 100 calcite (mols) to 1 siderite (mols). If, on the other hand, the conditions changed from a solution saturated with calcium and iron at A to one saturated with both compounds at C, no calcite would precipitate and the concentration of calcium would remain constant, whereas the concentration of iron would be reduced from 10^{-3} mols per liter to approximately 10^{-5}. Furthermore, no iron would precipitate until the siderite-hematite boundary was reached, and the iron mineral formed would be entirely hematite. If, on the other hand, a solution saturated at A changed in its characteristics to those at D, again no calcite would precipitate; instead, pyrite would form and would remove the iron until the residual concentration was less than 10^{-10} mols per liter.

[4] The "solubility of calcium carbonate," as plotted on this diagram, does not represent the true stoichiometric concentration of calcium carbonate—rather, it represents the activity of the calcium ion. The values relative to iron activity are correct, however, and the actual solubilities are approximately in the same ratio.

It is possible to follow the changes that would occur from almost any postulated set of conditions. For example, if the solution at *A* is saturated with iron (10^{-3} mols per liter), but only one-tenth saturated with calcium ion, and if the environment changes from that at *A* to *B*, siderite will precipitate as the solution goes from *A* to *B*, and no calcite will precipitate until *B* is reached. If the environmental characteristics continue to change toward *E*, siderite and calcite will precipitate together in the approximate mol ratio, 100:1.

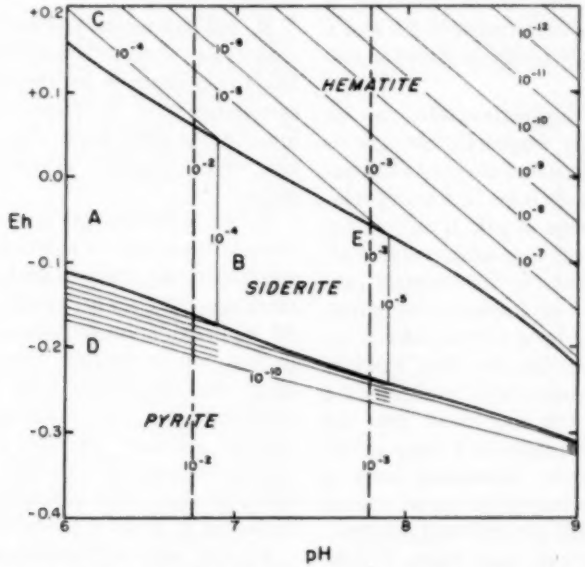

FIG. 3.—Stability diagram of hematite, siderite, and pyrite, showing activity of ferrous + ferric iron (*solid contours*) and activity of calcium ion (*dashed lines*) expressed in mols/liter. See text for letters *A*–*E*.

Thus from the diagram it is possible to investigate the various types of postulated conditions for iron deposits and for the formation of sideritic, pyritic, and hematitic limestones. One of the most important relations, perhaps, is that there is a relatively restricted range of environment in which significant concentrations of iron can be carried. These lie almost entirely within the range of environment shown by the siderite field, and especially by that part of the siderite field lying at pH values of 7 or less. Any solution with characteristics lying outside this range of composition cannot carry more than 10^{-4} mols per liter of iron. Presumably, then, iron must be transported in true solution in media with approximately these characteristics, whereas effective transport of calcium measured in the same way (with 10^{-4} mols per liter as a cutoff point) can take place anywhere on the diagram. It should be explicitly pointed out that this treatment does not include iron carried in colloidal suspension.

From the preceding relations, some understanding can be gained of the relative infrequency of occurrence of essentially pure sedimentary iron mineral

deposits, whether they be siderite, pyrite, or hematite. The transporting medium would presumably have to have a pH of 7 or less and an Eh in the range approximately −0.1 to +0.3. Furthermore, for precipitation to occur, the environment would have to change in such a way as to preclude the precipitation of calcium carbonate. In general, this means an increase in pH, accompanied by a marked increase or decrease in Eh and a solution originally undersaturated in calcium ion.

Some useful suggestions also can be made concerning diagenetic changes to be expected. The usual effect of diagenesis is to cause a decrease in Eh and relatively little change in pH. If an original deposit of hematite were subjected to such a change, so that the Eh dropped into the siderite field of stability, one might expect to find at least a little siderite replacing hematite. On the other hand, if the Eh change were drastic and caused a rapid drop all the way down into the pyrite field, one might find some pyrite replacing hematite. Relatively little is known, however, as to the speed of such changes in already precipitated material. Solubilities are low, and there is considerable doubt as to whether equilibrium relationships are attained. On the other hand, in original deposition from true solution, it seems probable that equilibrium would be maintained and that the mineral formed would be reasonably diagnostic of the conditions of precipitation.

CONTROL OF ION SPECIES BY Eh AND pH

The ways in which the various ions present in sea water have been shown to behave when subjected to changes in Eh or pH permit a fourfold subdivision of ionic behavior:

A. *Eh- and pH-independent species.*—The concentrations of these forms are not directly affected by the normal range of changes in either Eh or pH. They form no complexes with hydrogen or hydroxyl ion, hence are pH-independent; significant quantities of other valence forms can be obtained only by Eh values considerably higher or lower than those found in nature.

B. *pH-dependent species.*—The concentrations of these ions are highly sensitive to pH changes, for they form strong complexes with hydrogen or hydroxyl ions. On the other hand, they are insensitive to Eh changes within the natural range.

C. *Eh-dependent species.*—Concentrations of these ions in solution are highly sensitive to Eh changes within the natural range, but they are unaffected by pH. At high oxidation-reduction potentials, ferrous ion is largely transformed to ferric, and the ferrous form becomes an extremely low percentage of the total iron in solution. On the other hand, ferrous ion is not pH-sensitive, for it does not form significant quantities of soluble hydrogen or hydroxyl complexes.

D. *Eh- and pH-dependent species.*—Concentrations of these ions are controlled by variations in either Eh or pH. A good example is the sulfide-sulfate relation. The ratio of sulfide to sulfate increases with decreasing Eh and with decreasing pH. The reaction for the change can be written

$$S^{=} + 4H_2O = SO_4^{=} + 8H^{+} + 8e\ .$$

Thus the ratio is controlled in part by the oxidation-reduction potential and in part by the hydrogen-ion concentration.

Table 6 lists ionic behavior, and figure 4 shows the types of behavior in terms of a plane related to three mutually per-

pendicular co-ordinate axes. The species represented in diagram *A* have the plane parallel to the Eh axis, whereas those in *B* have the plane parallel to the pH axis. For species independent of either pH or Eh, the plane is parallel to the Eh-pH plane, as shown in diagram *C*. Finally, species dependent on both factors (diagram *D*) have the plane intersecting all three axes. The relations shown are highly generalized; *A* shows only that in a given solution carbonate or phosphate ions increase in amount as the pH goes up, whereas they remain constant, whatever their absolute values, if Eh is changed while pH remains fixed. The reason for the change is their release from hydrogen ion complexes as the hydrogen ion is decreased.

RELATIONS AMONG SEDIMENTARY ENVIRONMENTS IN TERMS OF Eh, pH, AND CONCENTRATION (SALINITY)

The mineral associations commonly observed in chemical sediments support the inferences in the preceding section regarding the dependence or lack of dependence of various chemical end-members on Eh, pH, and concentration of the sedimentary medium. Examination of limestones, black shales, and evaporites shows that the most common associations in each sedimentary type satisfy a common framework of Eh and pH.

MARINE LIMESTONE ASSOCIATIONS

The general class of "normal marine limestone" (Krumbein and Sloss, 1951, chaps. 5 and 6) includes light-colored, fine- to medium-textured, massive to well-bedded limestones, with sharply defined smooth bedding planes. Fossil fragments may be inconspicuous, although whole fossils are sometimes fairly plentiful. These limestones are believed to have formed by precipitation of calcite through chemical or biochemical processes in relatively quiet moderate-depth waters with open circulation. Dolomitization and chertification are common postdepositional changes.

Other limestones believed to have been formed under normal marine open-circulation conditions include fossiliferous-fragmental types (coquina, crinoidal limestone, foraminiferal limestone, biohermal limestone, and others). These may be cross-bedded, and they generally show evidence of selective sorting and abrasion of the fossil fragments. The inference is that the fragments were transported by currents before coming to final rest. They constitute part of the class of "clastic limestones" (Pettijohn, 1949).

The most abundant primary chemical end-member in marine limestone is calcite. In addition, there is an average of about 0.4 per cent iron, present as siderite, hematite, or limonite, and traces of manganese oxide and calcium phosphate.

TABLE 6

BEHAVIOR OF ION SPECIES AS FUNCTIONS OF Eh AND pH*

Ion	Dependent on Eh	Dependent on pH
Na^+	0	0
K^+	0	0
Ca^{++}	0	0
Mg^{++}	0	0
Cl^-	0	0
Br^-	0	0
I^-	0	0
CO_3^{--}	0	×
PO_4^{---}	0	×
OH^-	0	×
Fe^{++}	×	0
Fe^{+++}	×	0
Mn^{+++}	×	0
Mn^{+++}	×	0
Mn^{++++}	×	0
$S^=$	×	×
SO_4^{--}	×	×

* In this table the symbol × indicates dependence, and 0 indicates lack of dependence.

The principal secondary chemical end-members are dolomite and silica. The great abundance of calcite indicates a primary pH control during sedimentation. In the range of pH implied, iron would not be expected to precipitate at concentrations normally found in sea water. However, if sufficient iron ion is present, the ratio of calcite to siderite or hematite is about 100 to 1, which is of the same order of relative abundance as the average observed iron content of limestone. The writers are of the opinion that iron compounds are precipitated more commonly in relatively near-shore environments, where streams may supply the iron. Normal marine limestone formed in remotely offshore conditions presumably has little or no co-precipitated siderite or hematite.

The presence of pyrite, organic matter, and gypsum or anhydrite in normal marine limestones is very rare, unless introduced postdepositionally. Precipitation of pyrite and preservation of organic matter usually require negative Eh conditions, which are not characteristic of normal marine open-circulation conditions. Gypsum and anhydrite, on the other hand, require much higher concentrations than are present in normal sea water.

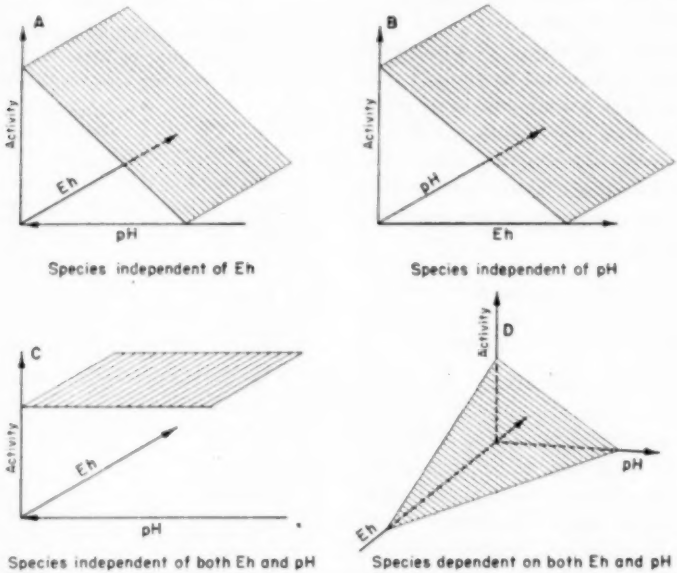

FIG. 4.—Diagrams showing various types of behavior of ion species with respect to Eh and pH. Note that positive direction of axes varies in the diagram. For species involved see table 4.

BLACK SHALE ASSOCIATIONS

Black shales as a class include a wide variety of chemical end-members, which may occur as interbeds or may be intimately mixed with detrital end-members in the thinly bedded or finely laminated shale. The most common chemical associations are calcite, calcium phosphate, pyrite, rhodochrosite, or-

ganic matter, and chert. When calcite is abundant, it implies pH of the sedimentary medium in excess of 7.8. The pyrite, rhodochrosite, and organic matter imply reducing conditions, with negative values of Eh. Phosphate is relatively Eh-independent and requires either higher concentrations than normal sea water or lower pH values than calcite deposition. The chert appears to be largely Eh- and pH-independent, because of its occurrence in a wide variety of sediments, as will be shown later.

Although both calcite and calcium phosphate may occur together in sediments, it is commonly true that phosphate-rich sediments are not highly calcareous. This suggests lower pH values than are required for calcite precipitation, and these lower values favor higher concentrations of phosphate ion. As long as the Eh is negative (but with pH higher than 7.0), the iron minerals may be siderite or pyrite (fig. 1), and the manganese minerals may be rhodochrosite or alabandite (fig. 2). With extremely high pH values, the higher oxidized states of iron and manganese may also be precipitated.

The general absence of hematite in euxinic deposits suggests that the pH usually does not exceed about 8.2. Where calcite is present in abundance, the pH was probably not lower than 7.8; but, where phosphate is abundant, the pH values may have been as low as 7.0.

The presence of chert in black shales and phosphatic sediments poses a problem which is not fully understood. It is possible that some occurrences of chert in pyritiferous deposits may be accounted for by the extremely low solubility of iron sulfide under highly reducing conditions. Under such circumstances, any iron present will be precipitated as sulfide, requiring precipitation of silica as chert. Chamosite, glauconite, or other silicate minerals may form under less reducing conditions or with less hydrogen sulfide. This possibility is implied in figure 3, which shows the steep drop-off on the pyrite side of the contour diagram as the Eh becomes more negative.

EVAPORITE ASSOCIATIONS

Chemical end-members in evaporites include gypsum, anhydrite, halite, calcite, primary dolomite, hematite, polyhalite, sylvite, and numerous others. Except for the first six named, however, all are comparatively minor in amount in evaporites of marine origin (Twenhofel, 1950). The diagnostic end-members in terms of Eh and pH are calcite, dolomite, and hematite, which imply alkaline and oxidizing conditions, although hematite may form with negative Eh values at high pH (fig. 1). It seems more likely to the writers, however, that convection currents due to temperature and density would distribute dissolved oxygen through the solution. The resulting Eh values, though positive, would be small, owing to the generally higher temperatures than prevail in the open sea.

A problem of considerable importance is introduced by the occurrence of evaporites in close association with black sulfide-rich muds or shales. Examples occur in the remains of evaporated lakes in Persia, where a salt crust overlies a black organic mud rich in sulfides (Ström, 1939, p. 363). Among ancient sediments, the evaporites and black shales of the Paradox formation (Pennsylvanian) in east-central Utah afford an example.

The critical influence of Eh on organic matter and sulfides, plus the critical influence of high concentrations on abundant organisms, suggests that the two conditions do not normally occur simultaneously in the same environment. It is

believed, however, that the apparent contradiction may be resolved by considering the position of the zero Eh surface (redox level) in the ancient environment. In normal open-circulation conditions the Eh is positive to the depositional interface and may become negative below. In this case, the zero Eh surface essentially coincides with the interface. In stagnant conditions the surface water may be normally oxygenated with positive Eh values, but the bottom water has negative Eh values. In this case the zero Eh surface lies at some intermediate depth. In some lakes the bottom mud has positive Eh values seasonally (Hayes and Coffin, 1951), in which case the zero Eh surface lies below the depositional interface.

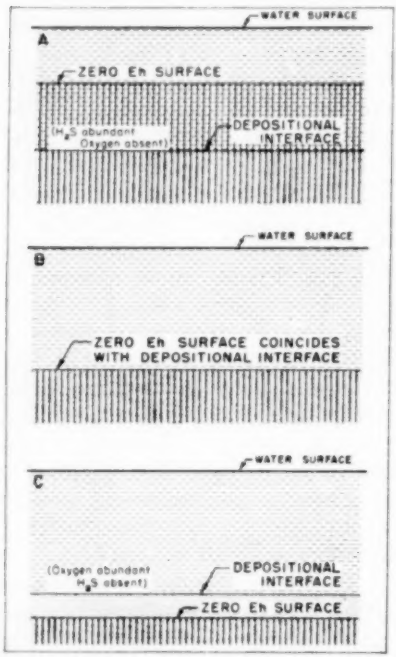

FIG. 5.—Diagram showing relations of zero Eh surface (redox level) to depositional interface.

Figure 5 shows these three conditions, in which the zero Eh surface lies above, at, and below the depositional interface. The position of the zero Eh surface is independent of concentration, in the sense that any of the three conditions may occur in fresh, brackish, and normal marine water, as well as in more concentrated solutions, depending upon restrictions on circulation or other factors which may limit horizontal or vertical currents.

The general absence among evaporite associations of chemical end-members which demand negative Eh conditions implies that the evaporating solution has positive Eh values, at least to the depositional interface. One may, however, postulate cessation of evaporative conditions in an evaporite environment due to changes in climate, whereby further evaporation essentially ceases. If restricted circulation continues, the zero Eh surface may rise above the depositional interface, producing negative Eh values in the bottom waters. Any organic matter which is washed into the environment tends to be preserved, and, if sulfate-reducing bacteria are active, sulfides may become associated with the black mud. In this way it appears possible that a special kind of euxinic environment, associated with higher than normal concentrations of dissolved salts, could exist and produce euxinic-type chemical end-members associated closely with evaporites. Conceivably, such alternating conditions could be recurrent.

GRADATIONS AMONG NORMAL MARINE, EUXINIC, AND EVAPORITE ENVIRONMENTS

The variable influence of changing Eh, pH, and concentration (total salinity) of the medium on the chemical end-members formed in sedimentary environments suggests that there may be a complete gradation from one extreme to an-

other. Figure 6 shows three mutually perpendicular co-ordinate axes representing Eh, pH, and total salinity. The axes intersect at Eh = 0, pH = 7.0, and salinity = 35 parts per thousand, which is approximately that of sea water. The two planes in the figure are the zero Eh plane and the neutral plane (pH = 7). These planes divide the space into four blocks, which represent oxidizing acid conditions in the upper left, oxidizing alkaline conditions in the upper right, reducing alkaline conditions in the lower right, and reducing acid conditions in the lower left. Each block extends from fresh water at the rear of the diagram to concentrated solutions at the front.

The several environments selected for discussion lie within the space represented by the diagram. The general class of reducing environments may be visualized as a three-dimensional solid lying below the zero Eh plane. This solid may extend from fresh water to highly concentrated solutions and may show some variations in its pH and negative Eh limits from one extreme to the other, depending upon concentration, temperature, and the nature of the ions present. The shape of the solid is not exactly discernible with present data, but both acid and alkaline conditions are known, and the Eh locally extends below −0.3. In similar fashion, open-circulation environments constitute a three-dimensional block lying above the zero Eh plane. At low concentrations this block may have a wide range of pH, but in natural environments it is believed that the solid would normally narrow to the right as it is followed from low to high concentrations, inasmuch as concentrated sea water tends to increase in pH. The upper limit of Eh would also change, partly as a result of temperature, which controls the amount of dissolved oxygen in the sedimentary medium.

Within the space represented by figure 6, it is possible to localize certain representative environments. This is shown in a simplified form in figure 7, which represents the same three axes as figure 6. The "signboards" in the axial framework show the average position of several sedimentary environments. The normal marine open-circulation environment has approximate limits of pH from 7.5 to 8.5

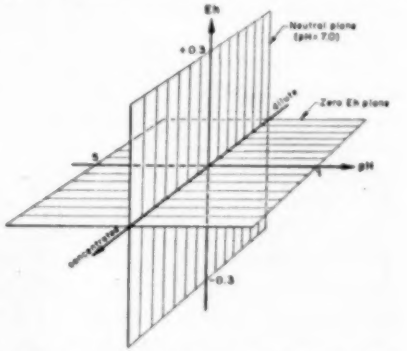

FIG. 6.—Schematic diagram of zero Eh plane, neutral pH plane, and salinity axis.

and Eh values from 0 to +0.2, as taken from table 1. The position along the salinity axis coincides with the intersection point, which was selected to represent normal sea water.

The average euxinic environment, based on values from table 2, has pH limits from about 6.8 to 8.2 and Eh limits from 0 to −0.3. The position along the concentration axis indicates slightly less dissolved salts than normal sea water. The evaporite environment is shown as an average position for halite-depositing conditions, with a concentration of the order of ten times that of sea water and with pH from about 8.0 to 9.0, as suggested in table 3. The Eh values are assumed to range from 0 to +0.1.

In the background of the figure is a "signboard" representing fresh-water bog conditions, with very low concentration of dissolved salts, with negative Eh values, and with a wide spread in pH.

In nature these average environments probably show all gradations, as was pointed out, but in the writers' opinion the difference associated with a change from positive to negative Eh conditions has a greater influence on the resulting associated chemical end-members than a change from acid to alkaline conditions.

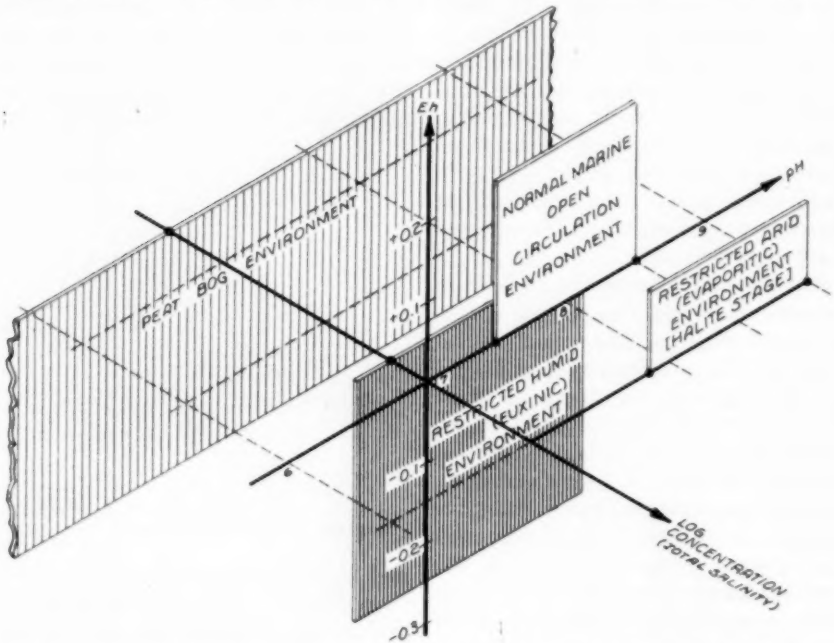

FIG. 7.—Generalized "signboard diagram," showing positions of selected environments in space represented by fig. 6.

INFLUENCE OF DIAGENESIS

Complexities are introduced into sediment interpretation by diagenesis. Below the depositional interface, oxygen is usually excluded, and any organic material present is attacked by anaerobic bacteria, commonly producing hydrogen sulfide (ZoBell, 1946). Consequently, even though the environment at the interface has a positive Eh, the presence of organic matter results in negative Eh values a few centimeters below. The pH, on the other hand, is not usually strongly affected.

From these relations it follows that the maximum effect of diagenesis can be expected where fairly rapid deposition takes place under conditions of positive Eh, with the entrapment of abundant organic material. Conversely, the minimum effect should occur in environments representing extremes from this optimum case: in euxinic environments, where conditions already are strongly reducing at

the depositional interface, so that burial produces no real change, and in deposits free, for one reason or another, of organic matter. When organic matter is absent, oxygen deficiency may occur below the depositional interface; but, even though the Eh becomes negative, there is no real reducing capacity because of the absence of organic material, and the original minerals are not changed. Iron deposits of the Clinton type may represent such a chemical sediment; the hematite was unaffected by burial because the original deposits were essentially free of organic matter.

Perhaps the maximum diagenetic effect occurs in the muds of the open-sea neritic environment under conditions of moderate rates of subsidence. Conditions at the interface are mildly oxidizing, but, with rapid sedimentation, much organic debris may accumulate. Chemical precipitates that were stable at the interface would tend to be reduced below. The total effect of the lowering of Eh is not well known. Among obvious results are the greenish spots around organic centers, where organic debris is sparingly present, and the development of pyritic replacements of plant remains.

Undoubtedly, the effects of diagenesis are much more far reaching, and many replacive relations observed between minerals of lower oxidation state and those of higher states may be the result of such diagenesis. Also, the development of abundant H_2S in the interstitial water of a buried bottom mud might tend to lower the normal pH of 8 by several tenths of a pH unit, forming a medium in which calcite would be unstable. Such a solution, if expressed through a more calcareous deposit, would tend to remove calcite, increasing porosity and permeability; it would tend to react with iron silicates and convert them to pyrite, perhaps freeing the silica and allowing it to be redeposited as replacive chert. These are highly tentative suggestions of some of the multiple complex reactions that might occur, but it seems likely that the typical marked decrease in Eh and the slight decrease in pH may provide at least a working basis for a detailed investigation of such puzzling diagenetic changes as chertification, dolomitization, and pyritization.

The writers believe that it is usually possible to distinguish between primary chemical end-members and diagenetically introduced minerals, although in some cases the distinction is difficult to make. On the whole, experience has shown that diagenetic effects of sufficient magnitude to mask the original Eh-pH conditions of sedimentation are relatively rare among the large bulk of common sedimentary rocks. They have been most puzzling in the little-understood sediments, which, in general, were those involving highly reducing or highly oxidizing conditions during or after formation. As the previous discussion has pointed out, however, the writers are of the opinion that the associations found in typical euxinic deposits are almost entirely primary.

CLASSIFICATIONS OF CHEMICAL SEDIMENTS

The chemical sediments are commonly classified according to composition, with subdivisions into siliceous, ferruginous, phosphatic, manganiferous, and other types. A necessary consequence of this subdivision is that chemical end-members which occur in similar environments are treated under separate headings, and the relations among them may be difficult to discern. Other classifications are based on genetic considerations, with the sedimentary environment or the

depositional agent as the main basis for subdivisions. In some instances genetic classifications are weakened by lack of sufficient data on sedimentary environments to permit complete interpretation of ancient chemical rocks. Current classifications are given in Pettijohn (1949), Twenhofel (1950), and Krumbein and Sloss (1951).

Increasing recognition in recent years that the Eh and pH of the sedimentary medium are important controlling factors in the formation of chemical sediments provides a logical framework for classification. This aspect has been explored by Rankama and Sahama (1950, p. 198), who propose the following geochemical classification of sediments modified from Goldschmidt: (1) resistates (undecomposed materials forming gravel and sand); (2) hydrolyzates (partly or wholly decomposed fine material forming bauxite, clay, etc.); (3) oxidates (ferric and manganic hydroxides formed in oxygenated lakes and seas); (4) reduzates (sulfides and other deposits formed in stagnant environments); (5) precipitates (sediments formed by inorganic precipitation when the solubility product is exceeded); (6) evaporates (sediments deposited from evaporating solutions); and (7) bioliths (sediments formed by organic processes).

The chemical sediments as treated in the present paper include classes 3 to 7, inclusive, of the above classification. This classification requires separation of limestone formed by purely inorganic reactions (class 5) from those formed by organic agencies (class 7), as Rankama and Sahama themselves point out. The present writers do not make the distinction between inorganic and organic chemical sediments but refer them to common groups on the basis of Eh and pH. Thus the bioliths, which include organogenic limestones, phosphatic sediments, diatomaceous shale, coal, and bitumen, can be referred to normal open-circulation conditions in part and to reducing environments in part.

A reclassification of the chemical sediments, based upon Eh, pH, and salinity (concentration) of the solution, may be developed in terms of positive or negative Eh, acidity or alkalinity, and dilute or concentrated solutions. If major rank is given to Eh, the two main classes would represent oxidizing and reducing conditions. Second rank may be given to pH, with two classes divided at pH = 7.0 or at some other selected values. Further subdivision is then possible in terms of salinity, with perhaps three or four classes representing fresh water (salinity = 0.1 parts per thousand $\pm$); brackish water (salinity = 10 parts per thousand $\pm$); normal sea water (salinity = 35 parts per thousand $\pm$); and hypersaline solutions (salinity = 200 parts per thousand $+$).

The organization of such a classification would be as follows:

I. Eh > 0
  A. pH < 7
    1. Fresh water
    2. Brackish water
    3. Normal marine water
    4. Hypersaline solutions
  B. pH > 7
    1–4 as above

II. Eh < 0
  A. pH < 7
    1–2 as above
  B. pH > 7
    1–4 as above

This organization, though logical, does not exactly fit common environmental limits. The writers prefer a semigraphic tabulation of chemical end-members based on certain natural limits of occur-

rence. These natural limits are in part suggested in figures 1 and 2 and include various modifying factors, such as the normal pH limit for formation of abundant calcite and the normal Eh limit for preservation of organic matter. By setting up selected "fences" on an Eh-pH diagram, the environmental categories, as represented by typical associations of chemical end-members, can be segregated on a more natural basis.

FIELDS OF OCCURRENCE OF CHEMICAL END-MEMBERS

Figure 8 is a diagram showing the fields of occurrence of typical chemical end-member associations, in terms of Eh and pH. The main blocks in the diagram are limited by pH = 7.0 and 7.8, by Eh = 0, by a sloping line which separates the iron and manganese oxides from the carbonates, and by a sloping line separating the sulfates from the sulfides. The boundary at pH = 7.8 is designated as the "limestone fence" because it is the approximate lower pH limit for the formation of abundant calcite. The zero Eh line is the "organic-matter fence," and the other limits represent "fences" between certain oxides and carbonates, as well as sulfates and sulfides.

The boundaries of the ten fields in the diagram serve as approximate limits to the associations shown. These associations, expressed in terms of the most abundant end-members (large capital letters), end-members present in appreciable amount (small capitals), and the accessories (lower-case letters), represent the expected groupings formed under each set of defining conditions. The diagram is based on the assumption that the average composition of the sedimentary medium is like that of sea water, although the diagram applies over a wide range of salinity, essentially from fresh water to hypersaline solutions.

The upper right field, limited by the limestone fence and the organic-matter fence, represents normal marine open-circulation conditions. Calcite is the most abundant end-member, and the minerals shown in smaller type are in the nature of accessory minerals, present in very small amounts. If this environment becomes concentrated to a salinity exceeding about 200 parts per thousand, the end-members shown in the brackets begin to precipitate as evaporites. This evaporite association, including primary dolomite, is believed to be the characteristic evaporite grouping formed under intra-cratonic basin conditions in a restricted arid environment.

The next field below the upper right represents euxinic environments with partial stagnation. The field is limited by the limestone fence, the organic-matter fence, and the Fe-Mn oxide-carbonate fence. The limestone formed in this environment, while still dominant, is partly bituminous but does not contain primary pyrite. As the succeeding fields below this are encountered, the Eh becomes more negative, the amount of organic matter may increase, and primary pyrite becomes increasingly important. The changing conditions are reflected in the diagram by the shift in emphasis among the several end-members shown.

If stagnation occurs in an environment after the salinity exceeds about 200 parts per thousand or if some evaporating environments have negative Eh conditions in the bottom waters, the resulting evaporites will presumably show increasing organic content and sulfides. The bracketed environments in the three lower fields on the right are tentative only, inasmuch as the writers have been unable to find critical information on the

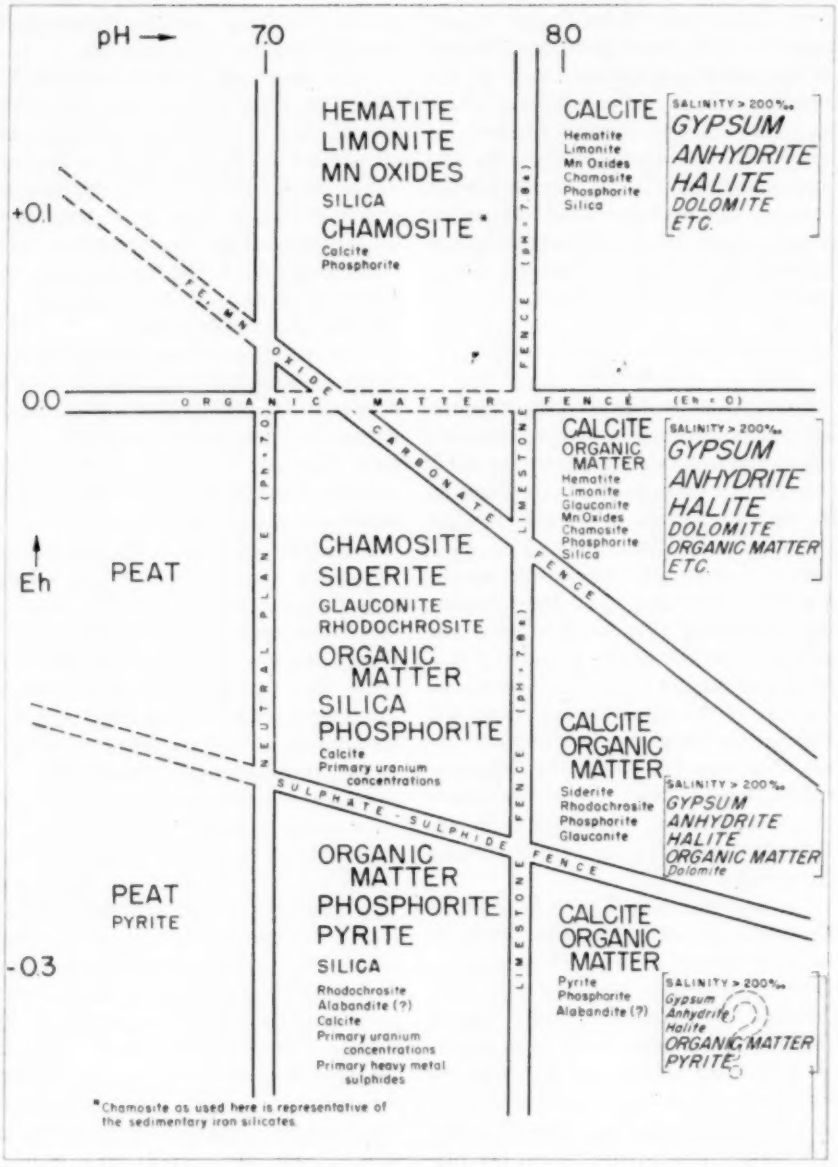

FIG. 8.—Sedimentary chemical end-member associations in their relations to environmental limitations imposed by selected Eh and pH values. Associations in brackets refer to hypersaline solutions.

occurrence of such rather special environmental conditions. Ström (1939) mentions that salt lakes and lagunas within the tropics almost always have foul bottom waters. Limitations imposed on living organisms by highly saline conditions may be overcome by the washing-in of plankton across barriers, which would supply organic matter to the environment. Similarly, if mud is washed in, the chemical end-members may be diluted with fine-grained clastic material.

In contrast to the fields located on the right of the limestone fence, chemical sediments formed in environments with pH between 7.0 and 7.8 tend to have calcite present as an accessory rather than as a dominant mineral. In the uppermost field in this group, with positive Eh values, the iron oxides and silicates predominate. As the Eh of the environment becomes negative, however, phosphorite and organic matter become increasingly important. Pyrite becomes abundant below the sulfate-sulfide fence, and presumably silica is also abundant there.

The fields at the left of the diagram represent acid environments. The writers do not know of deposits associated with such environments in the oxidizing realm. Below the organic-matter fence, and especially in dilute solutions, peat is the typical "chemical" end-member. Peat also can occur to the right of the pH 7 fence but is included in the blanket term "organic matter."

The influence of total salinity on the chemical end-members is apparently not great until high concentrations are reached. That is, the groups of end-members shown in the several fields are not significantly changed from fresh water through brackish water to the normal salinity of sea water. For the upper right group, calcite is still the dominant end-member in fresh-water environments, and one or more of the accessories shown may be formed along with it. True, the texture and faunal content of the fresh-water limestone will differ from its marine counterpart, but, as far as the chemical end-members themselves are concerned, absolute salinity seems to have only a slight effect on the association.

In a sense, therefore, figure 8 is a projection of figure 6 onto the Eh-pH plane, with the salinity axis compressed to a point at the intersection of Eh = 0 and pH = 7. Because of the change in end-members introduced by high salinity, the evaporite associations are shown in brackets. It is noteworthy that most of the evaporites are essentially Eh-independent and, according to theory, would extend into both positive and negative Eh environments, in solutions apparently somewhat more alkaline than normal sea water.

The graphic classification shown in figure 8 is for environments which are related more or less directly to marine water. The water may have normal salinity, it may be diluted with fresh water to produce various degrees of brackishness, or it may be concentrated by evaporation. In contrast to this "family" of environments are others which include waters of markedly different original composition, such as chloride, sulfate, and carbonate lakes (Clarke, 1924, chap. 5). Presumably in each of these different basic types, the relative abundances of chemical end-members would differ from those treated in this paper. It is believed, however, that similar Eh-pH-salinity diagrams could be prepared for them, and there seems to be no reason why the several "fences" shown in figure 8 would not apply to them as well.

EXAMPLES OF NATURAL MINERAL ASSOCIATIONS

The preceding classification of sedimentary chemical end-member associations was built to a very large extent on

theoretical relationships and represents predicted associations. Preliminary examination of natural occurrences indicates that they do fit the pattern, but much additional work remains to be done, and the validity of the groups will rest upon their general applicability. A few examples representing different environments have been chosen to illustrate the interpretation of depositional environments from natural mineral associations. Several of the examples are from economic deposits of chemical sediments. The use of these minerals as criteria of the depositional environment is not restricted to such highly concentrated deposits but is equally applicable when they are present only as traces. A few grains of primary pyrite in a shale or sandstone or a very low ratio of calcite to siderite, even though both are present in minute amounts, may be as revealing as if they were present in abundance.

1. *Huronian iron formations in Michigan.*—James (1951) classified the iron-rich rocks of the Iron River district of the northern peninsula of Michigan into four types; sulfide, carbonate, silicate, and oxide facies.

The sulfide facies is a graphitic, highly pyritic slate, with as much as 40 per cent pyrite and 10 per cent organic carbon, mainly graphite. The abundant sulfide and carbon and the absence of calcite put this rock in the pH 7.0–7.8 range and below the sulfate-sulfide boundary. James suggests exactly such an environment, except for direct stipulation of the pH, and calls for a stagnant-basin deposit.

The carbonate facies is chiefly siderite and chert. James has calculated the mineralogic content as including 62 per cent carbonate, 32 per cent chert, 2 per cent graphite, and the remainder small amounts of phosphate, chlorite, etc. The presence of abundant siderite, a smaller amount of graphitic material than in the pyrite facies, and the small percentage of calcium carbonate place this deposit in the block between pH 7.0–7.8 and between the organic-matter fence and the sulfate-sulfide fence. Traces of pyrite indicate a position low in the block. The pertinent conditions of deposition, as interpreted by James, fit well: "Bottom ventilation adequate to remove most of the organic material. It is to be noted, however, that reducing conditions still prevail, as attested independently by the presence of nearly 2 per cent graphite."

The silicate facies consists of siderite, magnetite, iron chlorite, and chert. James suggests that this mineralogy is the metamorphosed equivalent of a siderite-chamosite rock. Such a mineralogy would place it in the same block with his carbonate facies, but presumably above the organic-matter fence. This position is indicated by the absence of organic matter (or graphite) in the rock analysis, the absence of pyrite, and the presence of iron silicates, which occur elsewhere seemingly in equilibrium with hematite as well as with siderite. It has been suggested (Castano and Garrels, 1950) that chamosite is stable under oxidizing conditions at pH values slightly lower than those for optimum precipitation of hematite.

The oxide facies is not discussed in detail in James's paper, but it consists almost entirely of banded hematite and chert. James (personal communication) considers this facies, as seen in the upper part of the Negaunee formation in the Marquette trough, a primary sediment. This association also falls into the uppermost block between pH 7 and 7.8.

In summary, these four facies all fit into the predicted associations. Furthermore, there is a broad progressive rela-

tion between their stratigraphic position and the Eh of their environments of formation. The sulfide facies is at the base of the iron formation at Iron River, the carbonate facies is above, and the silicate facies is youngest, corresponding to a progressive increase in oxidation tendency, which implies removal of restrictions as time went on. There is no comparable sulfide facies in the Marquette area, but the lower part of the Negaunee iron formation can be classified as carbonate facies, whereas the upper part is an oxide facies, showing the same type of upward increase in Eh.

2. *Manganese deposits at Manuels Brook, Newfoundland.*—Dale (1915) described a section of Cambrian sediments on the southeast shore of Conception Bay in Newfoundland with a high percentage of chemically precipitated minerals. Shaly beds contain alternate concentrations of rhodochrosite, phosphorite, and pyrite. The pyritic beds are rich in organic material, the manganiferous and phosphatic beds much less so. Presumably, conditions alternated between those of the lower block between pH 7.0–7.8 and the block directly above, with the pyritic beds forming at lower Eh values and the manganiferous and phosphatic beds at the higher ones. The entire association implies negative Eh values throughout.

3. *Clinton iron ores of New York.*—The Clinton ores are composed chiefly of hematite and chamosite (Alling, 1947) and are intimately associated with much calcite. This mineral association places them in the upper field between pH 7.0 and 7.8. The abundant calcite suggests that the environment was to the right of the calcite fence much of the time. There is a suggestion of the same kind of increase in Eh through time as was noted in the Michigan iron ores, inasmuch as some siderite has been reported at the base of the ore beds.

4. *Phosphoria formation of Wyoming and Idaho.*—The oölitic phosphorite beds and phosphatic shales of the Permian Phosphoria formation presumably would fall into the field between pH 7.0 and 7.8, just below the organic-matter fence. Organic matter is sparingly present in the phosphorite (Mansfield, 1927), suggesting that the deposit was formed under conditions of negative Eh. Even though the organic matter is not abundant, conditions may have been rather strongly reducing, for the rate of deposition was exceedingly slow, according to Mansfield. Calcite is sparingly present in the phosphorite proper; other "indicator minerals" are surprisingly rare, however, so that the exact position of the environment is somewhat equivocal. The normal marine limestones and phosphatic limestones of parts of the Phosphoria suggest an environment with pH greater than 7.8 and with positive Eh values. Apparently the phosphorite of the Phosphoria represents restrictions on circulation, with development of lower pH values and a negative Eh condition.

5. *Examples of common normal marine and evaporite associations.*—It is interesting to note that the great bulk of the common chemical end-members of sediments appear to have been formed to the right of the limestone fence in figure 8. Nearly any fossiliferous-fragmental limestone association could be cited as an example of the uppermost field on the right of the diagram. Similarly, it is believed that the bulk of the intra-cratonic basin evaporites (Krumbein, 1951), such as the Silurian of the Michigan Basin, were formed in the same field under more highly saline conditions. The dolomite listed with the evaporites in figure 8 is considered to be primary.

Some slightly bituminous limestones and some glauconitic limestones appear to be representative of the field with pH greater than 7.8, and with Eh negative, between the organic-matter fence and the iron oxide–carbonate fence. Whether such limestones actually require negative Eh conditions in the medium directly above the bottom deposits is speculative; presumably very slight conditions of restricted circulation could swing the normal marine open-circulation environment into this field. The environments which are most troublesome to the writers include the several evaporitic-euxinic environments in the lower right of figure 8. The Paradox formation of eastern Utah may prove to be an example of one of these fields.

Occurrences of pyritiferous organic limestones, such as the Greenhorn of Wyoming (Rubey, 1930), also pose problems of environmental interpretation. As Rubey pointed out, the limestone has foraminiferal tests, which implies normal marine shallow-water conditions. The organic matter and pyrite, on the other hand, require negative Eh conditions. Rubey suggested an open-sea environment of rapid burial, which permitted preservation of organic material and fostered development of sulfides. In a sense this interpretation introduces a strong diagenetic environment for the pyrite. It is interesting, however, that the lowermost class on the right of figure 8 shows such an association as a possible primary one. It seems more likely, however, that in such cases the calcite would be a chemical precipitate rather than foraminiferal.

6. *Examples of euxinic associations.*—The typical euxinic environment, as represented by Norwegian fiords, would lie to the left of the limestone fence and below the sulfate-sulfide fence. A number of organic-rich pyritiferous black shales may be cited as examples of these environmental conditions. Typical are the many thin black fissile shales of the Pennsylvanian cyclothems, which have organic matter, pyrite, some phosphate, and occasionally some apparently primary silica. The very extensive black shales in the geologic column, such as the Chattanooga, appear to represent a range of conditions in the fields between pH 7.0 and 7.8 and below the organic-matter fence. Diagnostic minerals which may aid in more detailed environmental analysis of such shales are siderite, glauconite, pyrite, and the relative abundance of calcite.

CONCLUDING REMARKS

The present paper is an outgrowth of numerous discussions held between the writers and their colleagues at Northwestern University, especially Messrs. Dapples, Howland, and Sloss. It became apparent from the discussions that many of the interests of economic geologists coincided exactly with those of sedimentary petrologists and stratigraphers, as far as the environments of origin of sedimentary ores and sedimentary rocks are concerned. Study of the chemical end-members appeared to offer a common avenue of approach to some of the problems involved.

The recent great expansion of literature on Eh and pH, which is scattered through a number of scientific fields, convinces the writers that they have missed many pertinent publications. It is hoped, however, that enough was seen to set a proper framework for the present paper. The apparent lack of critical data on some special kinds of environments may be a reflection of the writers' failure to find some publications rather than an

index of ignorance on the subject among geologists, biologists, chemists, oceanographers, and the many other interested workers.

The principal feature which impressed the writers as work developed was the strong dependence of geological environmental interpretation upon certain laws of physical chemistry. By means of these laws it becomes possible to predict the mineral associations which should be found in a wide variety of sediments. In the actual study of sedimentary rocks, however, the strictly geological problem of distinguishing among primary and secondary sedimentary minerals is not so simple as theory would suggest. The writers have not intentionally slighted this aspect of the problem but have tried to concentrate on primary occurrences in this initial paper.

It was not possible completely to review the rich literature on mineral associations in sedimentary rocks, especially the minor constituents in mixed chemical and detrital sediments. It is recognized that some of the most diagnostic minerals may be among the minor constituents. Hence the framework for chemical end-member classification shown in figure 8 is offered more as a progress sheet than as a finished project. The several "fences" erected in the diagram may not be the most significant; the idea itself grew out of an earlier paper by Cox (1946) on the geological fence in the formation of petroleum. It seemed logical to see whether such natural limits might play a role in establishing certain diagnostic mineral assemblages. It seems not unlikely that some environments may have a sufficient range of Eh and pH conditions to straddle several of the fences. The influence of seasonal and other recurrent changes on these critical factors in environments may yield intimately interbedded deposits of apparently widely different origin.

Two problems of sedimentation which appear in a somewhat different light as a result of the present study involve the origin of primary chert and the question of evaporitic-euxinic environments. One of the present authors is on record as supporting the deep-water origin of primary chert, but it appears to be more a question of physicochemical conditions in the environment than of water depth. Similarly, it appears that, for full understanding of evaporites and all their associated sediments, the common segregation of environments into restricted arid and restricted humid is too general and involves extremes too great to account for associations which may require a common environment or alternate environments attained by slight changes in one another.

Additional problems which arose in the course of this work include the relation of diagenetic environments to primary sedimentary environments. The diagenetic environment associated with any primary environment may represent mainly a shift in position over the diagram of figure 8. That is, the mud bottom beneath neritic normal marine open-circulation environments may have the properties of the euxinic environment. Similarly, ancient sediments, exposed to subaërial agencies, may develop properties related to one of the oxidizing environments, such as that between pH 7.0 and 7.8, and Eh > 0. In the interpretation of these postdepositional changes, the presence of minerals from one field which show replacive relations to those of another markedly different field should furnish criteria by which the sequence of primary and secondary environments may be evaluated.

These several illustrations of addition-

al problems and areas in which further work is needed are examples of a wide range of topics which appear to be amenable to attack by analysis of Eh and pH controls on mineral associations. Applications of this type of analysis are by no means new, but it is believed that, by integrating these known principles with fields of mineral stability, problems which are now treated separately in sedimentation, economic geology, and other fields may be studied in a unified manner by collaborative effort. Mason (1949) has recently shown that this framework of thinking also extends into the broader geochemical cycles of the earth.

It seems worth while in closing to reemphasize the fundamental assumption of equilibrium conditions on which this paper is based. The broad correspondence between theoretical and observed occurrences as developed in the foregoing text suggests that this assumption provides a useful framework of reference. This framework should make it possible to assess those cases that show deviations from equilibrium conditions.

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EVALUATION OF CRITERIA FOR THE FORCIBLE INTRUSION OF MAGMA[1]

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ABSTRACT

Small Eocene rhyolite dikes in the Black Hills were forcibly intruded into steep-dipping pre-Cambrian schists and nearly flat Paleozoic rocks. Structural criteria for forcible intrusion are compelling in both rocks, but the full amount of deformation is measured only in the nearly flat strata. Larger dikes and stocks deformed the nearly flat Paleozoic strata, but deformational effects are difficult to detect in the steep-dipping schists, which suggests that criteria for forcible intrusion of magma into schistose rocks are poor, and negative evidence must be used with caution.

Structural criteria for the forcible intrusion of a body as large as the Sierra Nevada batholith are still more unsatisfactory. However, there are indications that this batholith was built up by multiple intrusions of a number of small intrusive units, each showing about as much deformation of the schist wall rocks as do the smaller intrusive bodies of the Black Hills.

An appraisal of evidence pertaining to forcible intrusion of magma indicates that it is probably not possible to measure the amount of forcible intrusion of a body as large as a batholith, but several lines of evidence can be interpreted to support the concept of forcible intrusions of batholith.

INTRODUCTION

Read (1948, p. 7), a leading proponent of the theory of formation of batholiths by replacement, said that "it is the problem of room that lies at the heart of the granitization problem, and the room problem is a matter to be dealt with by field geology." Structural relations between intrusive and intruded rocks can give a direct measure of the amount of granitization or of forcible intrusion of magma, thus offering a challenge to the structural geologist. An attempt will be made to test the validity of structural criteria of forcible intrusion of magma for intrusive bodies of different sizes. Few geologists seriously question the concept that some small dikes have made room for themselves by the forcible intrusion of magma; the difficulties faced by the alternative mechanisms of either creation of tensional openings or stoping of the wall rocks are often insurmountable. When larger intrusive bodies are concerned, however, many geologists will question the possibility of forcible intrusion, and, when we finally turn to the study of very large batholiths, we may find that the mechanism of forcible intrusion appears to face greater difficulties than those of magmatic stoping or replacement. The question of the forcible intrusion of batholiths is, therefore, an ever recurring one.

In the workings of the Homestake mine in South Dakota, small rhyolite dikes of Eocene age have been forcibly intruded into steep-dipping pre-Cambrian schists and nearly flat Cambrian strata, as is clearly shown by the structural relations between dikes and wall rocks. In adjacent parts of the Black Hills, large dikes and stocks have plainly domed and displaced the nearly flat Paleozoic strata on a large scale, but the steep-dipping pre-Cambrian schists which intervene between the intrusive bodies and the superjacent Paleozoic strata seem to have undergone much less deformation; so much so, that it seems doubtful whether forcible injec-

[1] Manuscript received October 30, 1950.

tion of the stocks could be convincingly demonstrated by a study of the schists alone. It must be concluded, therefore, that the structural criteria for recognizing forcible injection of intrusive bodies into steep-dipping, schistose rocks are rather unsatisfactory and that large-scale deformation of such rocks is difficult to detect unless, as is rarely the case, all three dimensions are exposed.

Excellent studies are available of the structure of the Sierra Nevada batholith in California, particularly in a recent paper by Mayo (1941) and in earlier ones by Ernst Cloos (1932, 1933). These studies seem to show that the Sierra Nevada batholith, like the dikes and stocks in the Homestake mine and the Black Hills, was built up by successive intrusions of many small, separate units. Although the structure of the schists surrounding the intrusive bodies does not show clearly how much of the space now occupied by intrusive rocks was gained by forcible injection of the magma, the internal structures of the intrusive bodies do suggest that the magma was forcibly injected, and the conclusion seems justified that large volumes of schist were displaced upward and were subsequently eroded.

An appraisal of some of the lines of evidence which have been used elsewhere in studying the question of the forcible intrusion of magma shows that, in the absence of measures of displacements in all three dimensions, the evidence is frequently conflicting and can easily be misconstrued. Stoping or assimilation of wall rocks, granitization or metasomatism, and forcible injection can operate concurrently; hence each process must be measured, and this measurement is a structural problem. Most important of all, however, and particularly pertaining to the Sierra Nevada problem is the question of the time relations. An apparently widely held opinion is that deformation which precedes intrusion cannot be caused by the intrusion, on the ground that the effect cannot precede the cause; but this is by no means true, and, in fact, deformation of a compressional character must, in general, precede the emplacement of the magma which causes it. It would seem, therefore, that there is much evidence for, and little evidence against, the forcible intrusion of the Sierra Nevada batholith.

FORCIBLE INTRUSION BY SMALL DIKES

In the workings of the Homestake mine in the Black Hills of South Dakota, a swarm of rhyolite dikes of Eocene age intrudes highly folded pre-Cambrian schists and nearly flat Cambrian sedimentary rocks, and many individual dikes in this zone show clear evidence of having been forcibly intruded into the enclosing rocks. Most of the individual dikes are about 10 feet wide and a few hundred feet long, but some are only a foot or two wide, whereas others are as much as 30 or 40 feet. A notable feature of these dikes is their limited length in plan. Only a few dikes extend all the way through the few hundred feet of explored ground on any one mine level, and probably most are only a few feet to a few hundred feet long, although the whole zone on the surface is about 3 miles long. Dike ends are thus exposed in many places in the mine workings. Figure 1 shows two examples of typical dike ends and the accompanying displacement of the wall rocks, the shape shown in figure 1, *A*, being more typical, as the dike ends commonly have a semicircular cross section. If wedge-shaped prongs project from the blunt ends, they are commonly at one

side or the other, as in figure 1, *B*, but they generally do not follow small faults. In the examples illustrated, the structure of the wall rocks is easily visible and shows clearly that the dikes were forcibly injected. If the area of intrusive rock is removed from the drawing, a void will be left which can be completely filled by bending the wall rocks back into place. More convincing proof of forcible intrusion can hardly be expected.

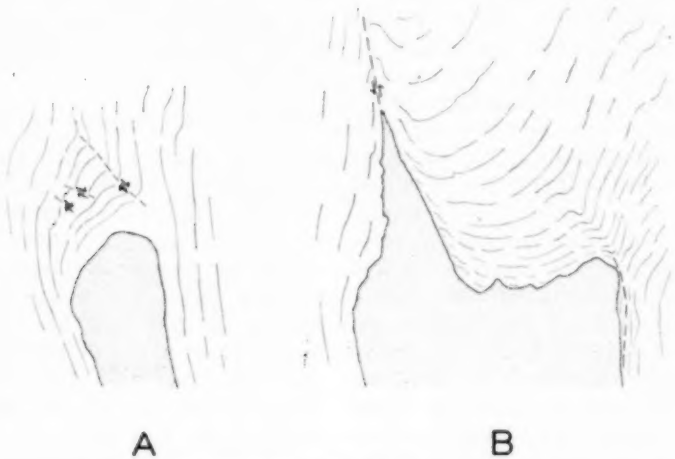

FIG. 1.—Deformation of schist at the ends of two dikes in the Homestake mine. Sketched on photographs. The schists have been thrust aside and ahead of the advancing magma and have been displaced along small faults. In *B*, the magma subsequently advanced along one of these faults. The dike in *A* is about 15 inches wide, that in *B* about 7 feet wide. *A* is a plan, *B* a vertical section.

Nearly as convincing proof of forcible intrusion by the dikes is furnished by a type of structure which can be called a "dike offset." The excellent exposures of many dikes in the mine workings have disclosed some interesting three-dimensional relations which are not visible on the surface. Any individual dike can be most clearly described by comparing it to the reader's left hand, so held that palm and fingers are rigidly extended in a plane, the fingers closely appressed and inclined upward at 20°–30° from the horizontal and the plane of the palm and fingers inclined toward the reader at an angle of about 65° from the horizontal. The palm of the hand then represents a single dike, which on a horizontal plane, such as a mine level, is commonly a few feet wide and a few scores or hundreds of feet long, having apparent, blunt-nosed ends. The real end of the dike is, however, diagonally upward, where the simple dike breaks up into a line of flattened fingers of elliptical cross section. As a whole dike was forced upward into the schistose wall rocks, the fingers continued to advance ahead of the main dike; but here the analogy to the human hand no longer applies because the tiny ridges and offsets which mark the junctions of dike fingers are retained in the walls of the main dike lower down. If the axes of the separate fingers are exactly in line, a small, sharp ridge of schist is preserved along each wall of the dike. Plate 1, *A*, shows two dike fingers

A

B

Intrusive features in the Homestake mine and open cut

which just failed to merge before final freezing. If the tiny line of schist fragments forming a parting screen between the two fingers had been scattered by the flow of the magma, only the schist ridges would mark the point of junction of the two fingers. More commonly, however, the axial planes of the separate fingers are slightly en echelon; and then merging of the fingers produces not a schist ridge but a pair of matching offsets in the dike walls. In figure 2 are shown successive stages in the development of dike offsets. As the fingers advance by prying the schists aside, when two fingers are about to pass (as in fig. 2, *B*) the schist is cut by the gigantic pair of blunt-edged scissors formed by the dike fingers. The magmas then mingle and disperse the fragments of the parting screen; but, as the walls are further separated by the continuing pressure and flow, the right-angled offsets are preserved to mark the place of junction of the earlier fingers.

To D. T. Griggs belongs most of the credit for explaining the mechanism of these dike offsets, and several of the photographs on which were sketched the data for figures 1 and 2 were taken under his direction. As Griggs (private report) has pointed out, the preservation of such features as the schist ridges ("brecia ridges" in his terminology) and the dike offsets is a strong argument not only for the concept of forcible intrusion but also against a concept of appreciable wall-rock assimilation or stoping. Delicate features like the ridges of schist shown in plate 1, *A*, are found on wide dikes where they could scarcely be expected to remain if assimilation of wall rocks was a universally important feature of emplacement of the dikes.

Although simple dike offsets like those of figure 2 are very common in the Homestake mine, there are also many variations from this simple pattern. Another common type, called a "dike-fault offset" by Griggs (1939), resulted when the displacement of the schists occurred along an actual joint plane rather than by the bending of the schists. The joint usually cuts the schists at a high angle to the bedding, and the dike noses abut against the joint, giving the impression to a casual observer that the dike is faulted; but no fault extends into the wall rocks. Another variation of the simple dike offset is formed by the merging of dike fingers of different widths. Adjacent dike fingers tend to be of about equal width because the magma columns are connected in depth and the pressure is essentially hydrostatic; but at times fingers do grow at

PLATE 1

A, Photograph showing nearly the final stage in the junction of two dike fingers in the Homestake mine. The exposure is on the wall of a crosscut, and the photograph is a vertical section. The motion of the magma was not directly upward, however, but was up the axes of the dike fingers, which here are inclined upward at about 25°; hence the motion of the magma was at a high angle to the plane of the photograph. A tiny screen of schist fragments can be traced between the dike fingers from one schist ridge to the other. The scale shown is 6 inches long, and the dike fingers are about 30 inches wide.

B, Dikes, schist, and Cambrian strata in the Homestake open cut. Northeast side of the Homestake open cut, viewed from the southwest. In the lower half are pre-Cambrian schists, isoclinally folded and dipping steeply northeast, away from the observer. The upper half of the exposure is made up of Cambrian strata (*dark gray*) and rhyolite sills (*light gray*), both dipping gently to the northeast. The sills were fed by dikes which cut the schists in the right side of the photograph.

different rates, owing to internal friction or to differences in external resistances. Where dike fingers of unequal widths merge, the resulting offsets, of course, lack the symmetry shown in figure 2, *C*.

These individual dikes form a dike swarm or zone in the Homestake mine, which is appreciably more regular than the individual units of the zone. Figure 3 is a map of the dikes on part of one of the mine levels. It shows the whole width of the dike zone at this place but not its full length, the zone probably extending for more than a mile each way along the strike, according to information available on the surface. Figure 3 shows some thirty separate intrusive bodies. Multiple contacts, with or without narrow parting screens of schist, are very common and show that the zone of dikes was built up by successive injections of magma. Most dikes have a few inches of flow banding along the walls, which makes it possible to map multiple

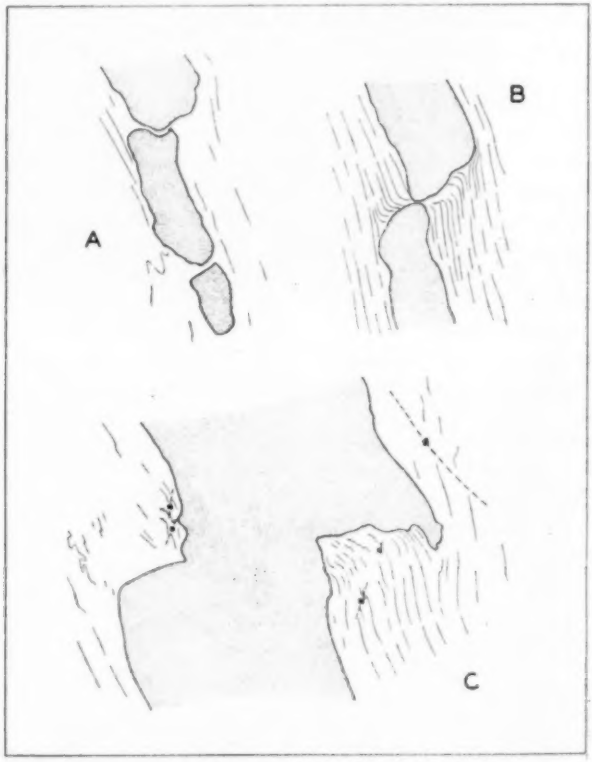

FIG. 2.—Deformation of schist at the junction of dike fingers in the Homestake mine. Sketched on photographs. In *A* are shown three separate dike fingers. In *B*, two dike fingers have nearly joined, and the schist is strongly bent to wrap around them. In *C*, the point of junction of two dike fingers is preserved by opposed offsets after the walls of the dike have been spread apart. Deformation due to the injection of the dike fingers is visible in the schists. The widest dike finger in *A* is about 1 foot; in *B*, about 2 feet; and in *C*, about 4 feet. *A* and *B* are plans, while *C* is a vertical section. The direction of flow of magma in all the dike fingers is diagonal to the projection plane in each case.

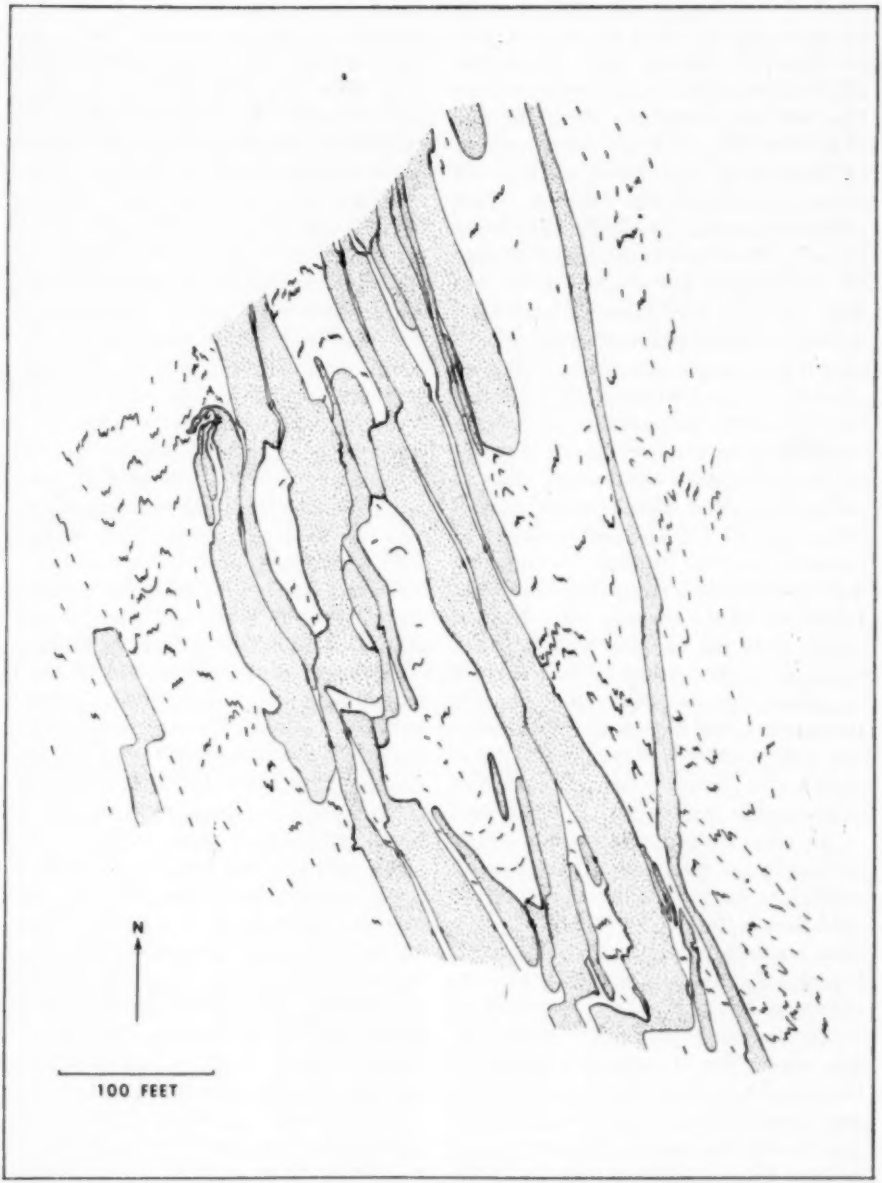

FIG. 3.—Map of the dike zone in a part of one Homestake mine level. Observed contacts are marked by hachures. Both the dikes and the schist wall rocks dip east, the dikes steeper than the schists. The schists and also the dike offsets and dike ends plunge to the southeast at about 30°.

contacts with certainty and in most cases to determine relative ages where two dikes are in contact. Younger dikes were injected most commonly along one wall of an older dike, in some places between two older dikes, more rarely at a distance of many feet from older dikes. Although no sure examples of younger dikes crossing older dikes have been seen, it is possible to interpret a few relations in that way; but examples must be very rare. No sure evidence has been found to indicate that younger dikes have deformed older dikes along whose walls they were injected, and in all cases it seems to have been easier for the new dike to displace the schist outward and upward than to deform an earlier dike. Displacement of schist toward the footwall is about as common as that toward the hanging wall; the relatively steep dip of the dikes (about 65° on the average, for individual dikes) gives the hanging wall little advantage. In the area of figure 3 there is no general age succession from one side to the other, but there is an indication—not well marked, however—that intrusions are successively younger each way from one dike near the center of the zone.

In plan the individual dikes are en echelon in the dike zone. The strikes of individual dikes are a little more to the west than is the strike of the whole dike zone, and offsets are usually to the right. Presumably this en echelon pattern represents a fracture pattern, possibly of tension fractures between counterclockwise shears; but it must be emphasized that the dikes we see were not guided by any fractures now visible. Whatever guided each dike was probably a minute, more or less incipient or potential, fracture, whose only remaining trace is the resulting shape of the dike itself. No branching or joining dikes have been found, and, in view of the abundant exposures, it can be assumed that none was formed. This would suggest that each dike was injected into a single plane of weakness; when that plane was completely utilized and no more magma could be injected, a new plane of weakness was then produced and utilized.

The schist structure shown on the map of figure 3 gives little evidence of having been displaced by the dikes. The schists had been greatly deformed in pre-Cambrian time, long before the intrusion of the dikes, and the deformation produced during the period of dike intrusion was merely added to the early deformations. Many examples of displacements of schist contacts at right angles to dike walls have been found on this and other mine levels; but, because the level maps are not true cross sections, the three-dimensional relations between the dip of the dikes and the dip and plunge of the schists reduce the amount of apparent displacement on the horizontal plane of a mine map. Reconstructed "pre-dike" structure maps, made by removing the dikes one by one and matching the walls, do not show the actual pre-dike structure because of bodily shifting of some enclosed segments of schists and also because weaker rocks were squeezed away from the dike zone and piled up at a distance. There are indications of unusual disturbances in the vicinity of the dike zone, but the details defy measurement. With no more information on schist structure than is shown in figure 3, the concept of forcible intrusion might be difficult to prove.

Fortunately, however, there is an opportunity to measure the displacement of another group of rocks: in the nearly flat strata of the Cambrian Deadwood formation. These strata rest unconformably on the pre-Cambrian schists and are well exposed in the large open cut of

the Homestake mine (see pl. 1, *B*, and fig. 4). As the dikes of the dike zone pass out of the schists and cross into the nearly flat superjacent strata, each dike displaces the base of the Cambrian strata in a direction at right angles to the walls of the dike. After crossing the base of the Cambrian strata, the dikes turn into the flat bedding planes and become sills; but some sills cut across bedding planes and earlier sills in places. Where planar features by the dike. Many examples have been found in the Homestake mine, and some are illustrated in figure 5. Though this relationship is very useful, there are certain limitations to its usefulness. In the first place, the plane of reference, whether a horizontal section on a mine map or a vertical section on a wall of a crosscut, must be approximately perpendicular to the planes of the intersecting features;

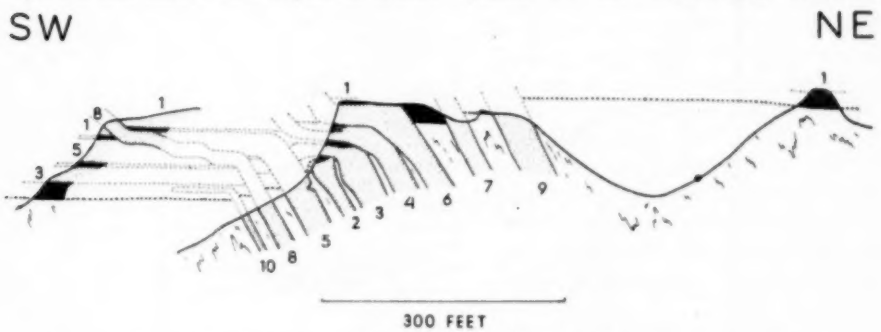

FIG. 4.—Displacement of the Deadwood formation on cross sections through the dike zone in the Homestake open cut. Dikes and sills are stippled. Pre-Cambrian schist is shown by schist structure symbols. Cambrian Deadwood formation is shown by solid black. The base of the Deadwood formation is displaced at right angles to the dike walls at each dike. The dikes and sills are numbered in approximate order of age.

this occurs, the displacement of the wall rocks is again at right angles to the walls of the younger sill. Even if the displacement of the base of the Cambrian strata were ascribed to "permissive" intrusion into tension fractures successively opened, the same explanation could not be applied to the displacement across the sills. The relations of dikes and sills to the Cambrian strata in the Homestake open cut are, therefore, compelling evidence that the dikes and sills were forcibly intruded. However, the schist structure shown in figure 4 is again noncommittal.

The ideal situation for measuring forcible intrusion of a dike is given by low-angle intersections of linear or otherwise the amount of displacement will be incorrectly shown. Oblique sections may give apparent displacements either greater or less than the real displacement. A second limitation on the use of offsets or diagonal features is that the lateral displacement does not extend indefinitely from the dike wall but must eventually be converted to upward displacement; and beyond the limits of lateral displacement the diagonal feature maintains its undeformed position. Possible relations are shown diagrammatically in figure 6. If the diagonal feature is really recognizable, as, for example, a contact rather than a bedding or schistosity plane, and if the exposures are good, as is generally the case

in mine workings, either of these types of displacement serves to measure forcible intrusion. If the diagonal planes are bedding or cleavage planes and hence not readily recognizable and if the exposures are poor, as is generally the case near intrusive contacts on the surface, then deformations of narrow width, as in figure 6, *B*, may be entirely missed, in which case the interpretation may be erroneously made that there has been no lateral displacement of the walls. Linear features, for example, crest lines of folds, may be displaced so as to give a measure of both upward and lateral displacement; these have been

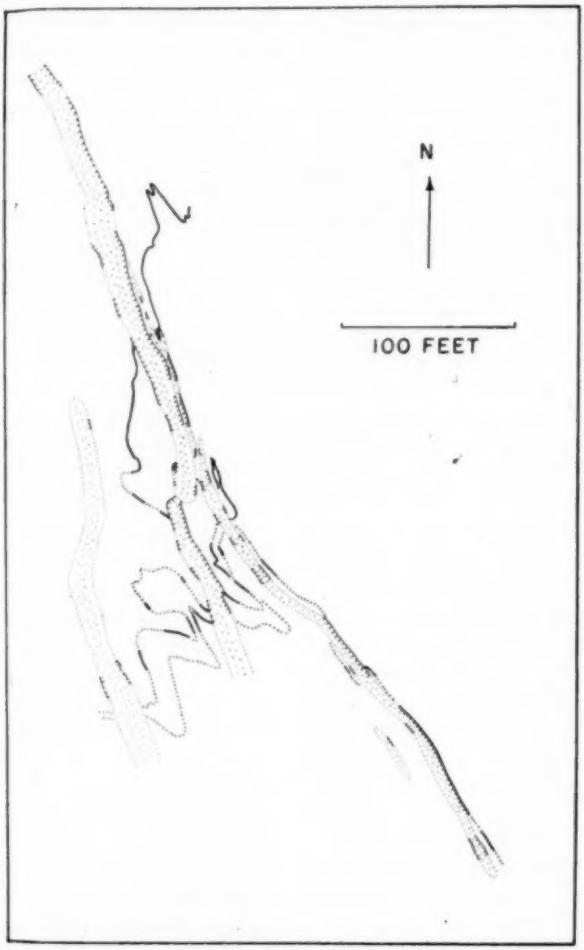

FIG. 5.—Contacts of the Homestake formation displaced across dike contacts in a direction approximately normal to the strike of the dikes. Hachured and solid contacts observed; dotted contacts inferred. Homestake mine, 2450-foot level.

found in some of the Homestake occurrences.

Some small-scale features of individual dikes suggest that these dikes have corroded or assimilated the wall rocks. At first this seems to contradict the concept of forcible intrusion, although the two processes need not be mutually exclusive. The evidence for assimilation of schist by magma of some dikes is chiefly a very ragged contact of dike against schist in which small portions of schist beds are actually missing. Extreme (not typical) examples are shown in figure 7. It can scarcely be questioned that some schist has been removed along the margins of these dikes.

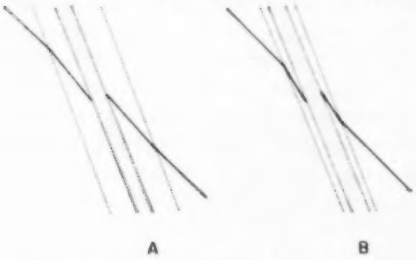

FIG. 6.—Diagrammatic maps of possible relations between forcibly intruded dikes and planar features intersected at low angles. In *A* the deformational effects are distributed over a zone wider than the dike; in *B*, over a much narrower zone. If the natural exposures are poor, relations like *B* are easily missed and may be interpreted erroneously.

Most of the Homestake dikes show an inch or a few inches of marginal flow banding, and this banding is especially striking in protuberances (fig. 7, *A*), which are most easily explained by assimilation. The flow banding is not caused by concentrations of light and dark minerals, there being no dark minerals in the dikes, or by changes in texture. In fact, the banding, though easily visible in specimens, is scarcely visible

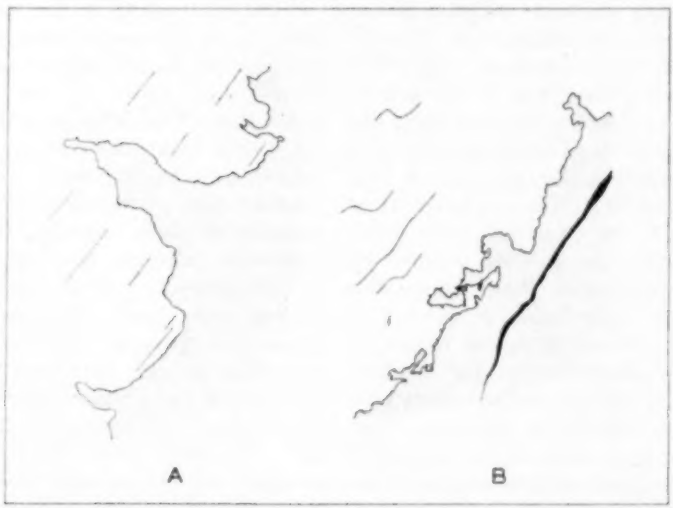

FIG. 7.—Examples of dike contacts in the Homestake mine which probably show some assimilation of schist by dike magma. Sketched on photographs. The nature of these contacts and the apparent disappearance of small amounts of schist probably show some assimilation of schist. The black line in *B* is a tiny breccia dike; fragments of porphyry and schist injected through the dike. In *A*, the straight-line distance shown along the contact is 3 feet; in *B*, it is only about 1 foot.

under the microscope and seems to be the result of an uneven dusting with minute inclusions. The indications are that the banding is caused by an uneven strewing-about of rejected material, mostly graphite, derived from the assimilation of a small amount of schist. If this explanation has general application, then most of the dikes have assimilated some schist, as most of them show marginal banding, but the amount of schist assimilated by any one dike may be very small.

The proof that the amount of schist assimilated must, in fact, be very small rests on the structural evidence of forcible intrusion already presented. Though the two processes need not be mutually exclusive, as there is no reason why a magma forcibly intruded into rocks could not assimilate or replace, actually the evidence for forcible intrusion sets rather definite limits to the possible amount of assimilation. The statement by Griggs, that preservation of the schist ridges and dike offsets is proof that there was no appreciable assimilation, has already been cited. The relations between Cambrian strata and dikes and sills in the open cut are even more compelling. The displacement of the base of the Cambrian strata is almost exactly the amount required by forcible intrusion. Any appreciable amount of assimilation or stoping of wall rocks would, of course, reduce by a corresponding amount the displacement of the base of the Cambrian strata. It can be concluded, therefore, that, although most dikes have assimilated or corroded their walls to a slight extent, the amount of wall rock so removed was very small, probably measurable in inches.

Internal structures other than the marginal banding are almost wholly absent from the dikes. The rocks are microgranular and micropoikilitic, made up of quartz, albite, and orthoclase or quartz and orthoclase. Some have sphene, but there are no ferromagnesian minerals. Chemically the dikes divide into two groups, one with high potash and the other with very high potash. The petrology of the dikes has been described elsewhere (Noble, 1948). There is nothing in the texture of the equidimensional grains whereby primary flow banding could be recorded. Jointing is, on the whole, rather uncommon. Two directions of joints have been noted in different places, one parallel to and near the dike walls and the other about at right angles to the axes of the dike fingers. Both types of fractures are probably caused by shrinkage on cooling.

FORCIBLE INTRUSION BY LARGE DIKES AND STOCKS

The dike swarm in the Homestake mine is part of a compact area of igneous intrusion of Eocene age in the Deadwood-Trojan uplift of the northern Black Hills (Noble, Harder, and Slaughter, 1949). Figure 8 shows this area of igneous intrusion and some of its structural relations. Although the small-scale features are very complex, the broad structural relations are fairly simple. Several large and small stocks of intrusive rock of very irregular outcrop shape are grouped together into a somewhat arcuate zone, and these are surrounded by a ragged fringe of individual dikes and swarms of dikes. The larger intrusive bodies have not been mapped in detail except around their edges, but reconnaissance traverses suggest that most stocks are made up of several intrusions of porphyry of different kinds and that parting screens of schist are fairly common. The stocks

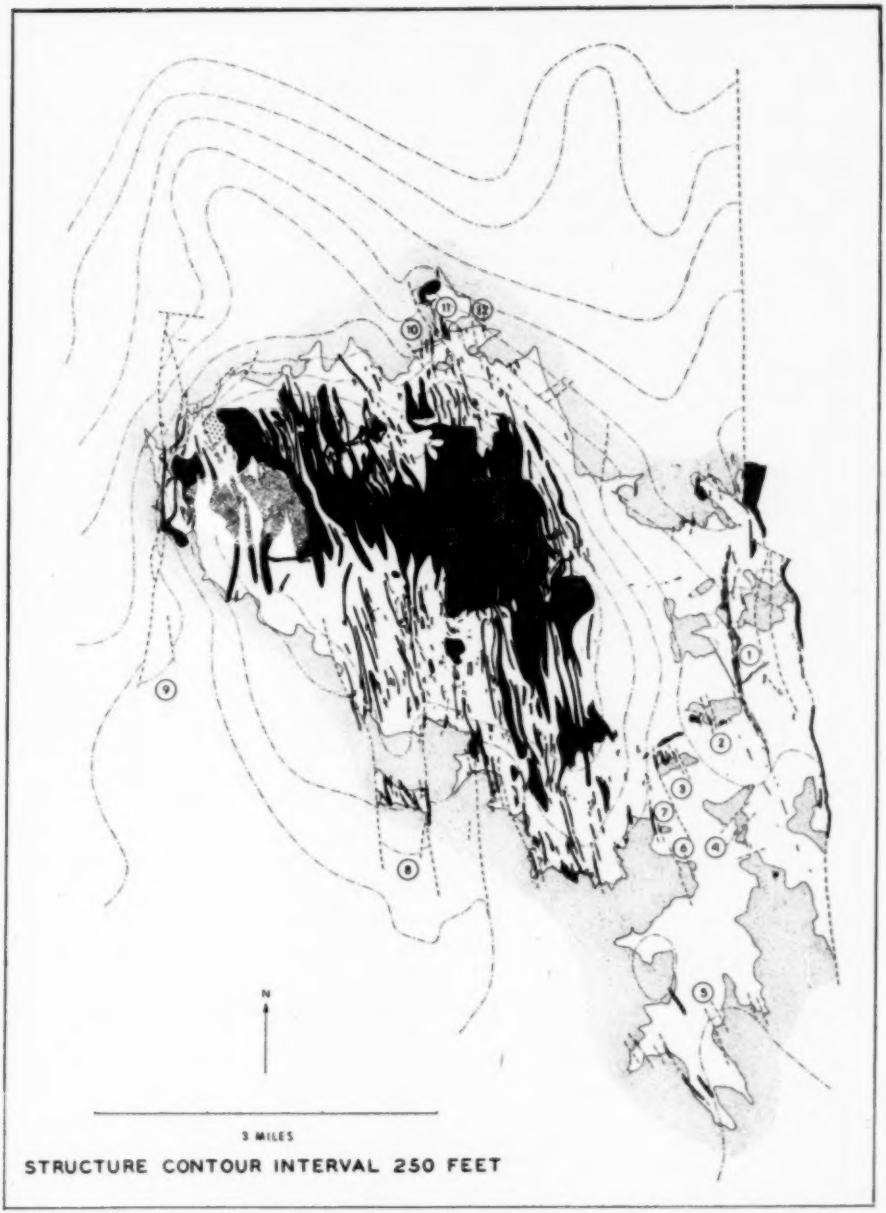

FIG. 8.—Deformation of the base of the Cambrian Deadwood formation around Eocene intrusive bodies in the Deadwood-Trojan dome. Intrusive bodies are shown by solid black; Cambrian Deadwood formation and other Paleozoic formations are stippled; faults are shown by dashed lines. Sills in Paleozoic strata are omitted. Pre-Cambrian schists surround the main area of intrusive rocks and are overlain unconformably by the Deadwood formation, but erosion has exposed them in a halo around the intrusive area. In schist near the western edge of the uplift, a small oval area (shown by small crosses) is a diatreme. Structure contours (shown by dot-and-dash lines) are drawn on the top of the schists. Contours drawn wholly over areas of schist show the minimum amount of deformation, and the uplift over the center may have been much steeper than is shown. Point *1* is the dike zone in the Homestake open cut; points *2–12* inclusive are "paired" faults.

and their satellitic fringes of dikes are intruded into pre-Cambrian schists; and resting unconformably on the schists is the Cambrian Deadwood formation. Structure contours drawn on the unconformity between Deadwood formation and schists make an elongated, slightly arcuate dome, which coincides almost exactly with the position of the arcuate zone of intrusive stocks and their satellitic dikes. The doming may have been even steeper than the map suggests, because the structure contours sketched where the Deadwood formation has been removed show the minimum possible displacement. The exact coincidence of doming and intrusion can be accounted for only by the doming's having been caused by intrusion. The alternative explanation—that doming localized intrusion—has nothing to recommend it.

The base of the Deadwood formation is crossed by a few small stocks and many dikes, and these make sills in the Deadwood strata; but it is obvious from a study of the map that many of the intrusive bodies stopped on approaching the base of the Deadwood formation. Faulting of the Deadwood formation is widespread and is, in general, in the same direction as the doming, the displacements being toward the higher part of the dome; but one type of faulting calls for special mention. This may be described as "paired" faulting, in which a block of ground has moved upward between a pair of vertical, nearly parallel, faults. The map (fig. 8) shows many examples of "paired" faults. In many of these, small porphyry dikes occupy one or both fault planes, and this relationship probably gives a clue to the cause of the displacement: the block of ground between the faults was probably lifted by a body of rising magma, which approached, but did not quite reach, the level of observation, except in the form of small prongs sent off along one or both of the bounding faults. This is an example of deformation caused by, but preceding, intrusion; the faults form ahead of the advancing magma and are intruded by the magma.

The structure of the pre-Cambrian schists is very complicated, but it has been studied in considerable detail, a study made possible by the presence of several good horizon markers. Figure 9, a map covering the same area as that of figure 8, shows the structure of the schists, the larger intrusive bodies, and the main faults mapped in the Deadwood strata; but the Deadwood formation is omitted. Close study of the map shows surprisingly little deformation of the pre-Cambrian structures, compared with the amount of deformation of the Cambrian strata already demonstrated. The most obvious deformations attributable to the intrusives are some thrusting-aside of the schists around the eastern side of the intrusive bodies and some deflections around small bodies south of the main intrusive area. For the most part, however, the schists maintain fairly constant strikes at a few degrees west of north. That forcible intrusion is not incompatible with undisturbed isoclinal strikes, unless linear features are considered, has been mentioned by Grout (1941); but in the present example the linear features, in the form of axial lines of folds, are of no help in testing the mechanism of emplacement of the intrusions. Throughout about one-fourth the map area, along the eastern side of the map, plunges of the isoclinal folds are to the southeast at angles ranging from 15° to 85°. The values recorded on the map are averages of many readings, and the

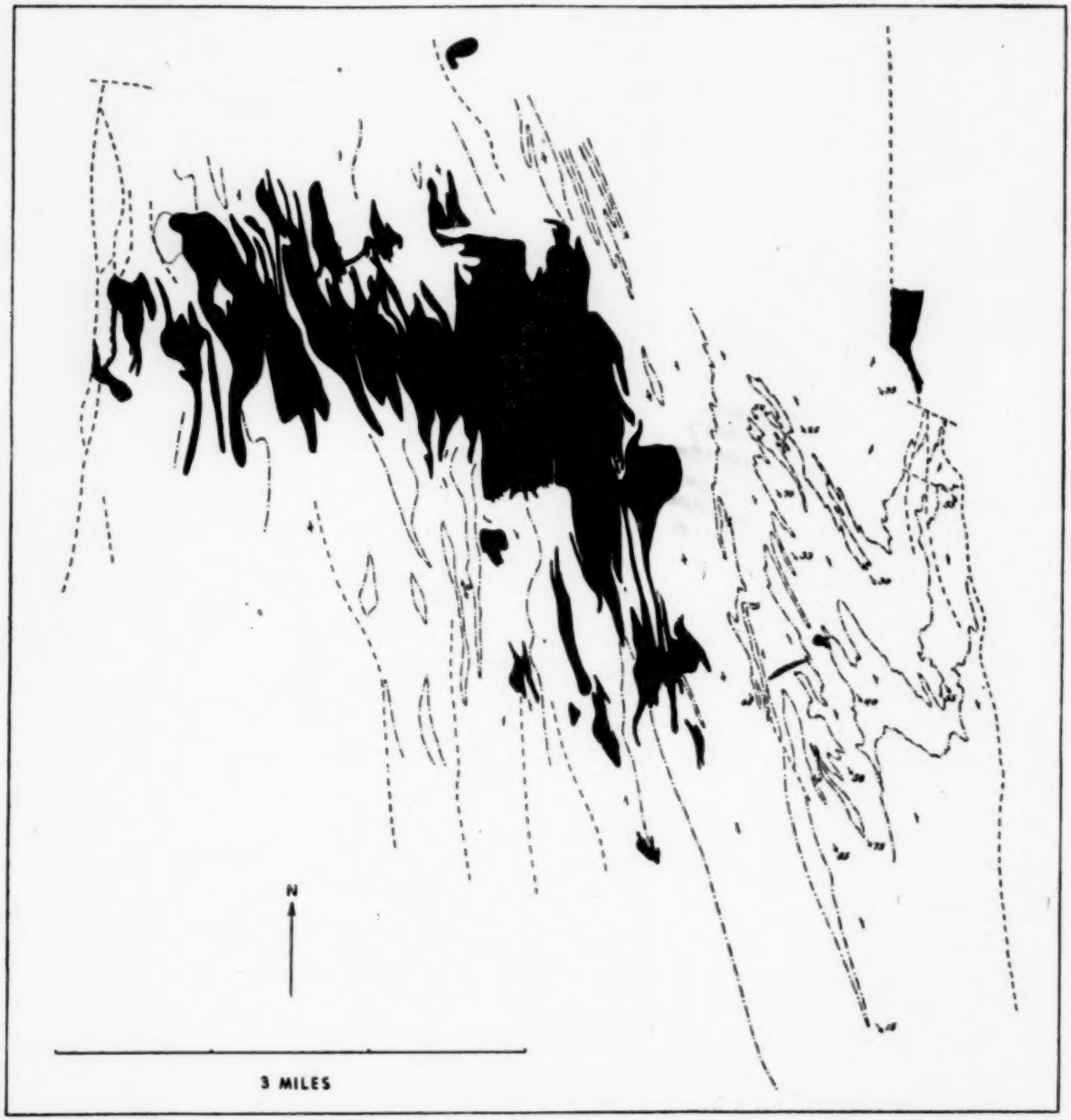

FIG. 9.—Deformation of the pre-Cambrian schists around Eocene intrusive bodies in the Deadwood-Trojan dome. Intrusive bodies are shown by solid black (smaller bodies omitted); contacts between formations in the pre-Cambrian schists by dot-and-dash lines; faults by dashed lines. Paleozoic formations are omitted. With the exception of one horizontal fold axis, no axial lines could be found in the western part of the area. The area of this figure is the same as that of fig. 8.

local variations are in places very great. Because the Cambrian strata in this part of the map are nearly flat, these structures date from pre-Cambrian time. In great contrast to this eastern part of the map, the rest of the schist structure shows practically no plunges whatever. At a single point, south of the main intrusive area, isoclinal folds with horizontal axes were recorded, but elsewhere no attitudes could be observed. The most reasonable conclusion is that such isoclinal folds as exist in the central and western parts of the area have approximately horizontal axes, as it is almost certain that steeply dipping axes would have been noted. Gruner (1941) has noted the difficulty encountered elsewhere in detecting isoclinal axes of low plunge.

Here are two maps, then, of the same area, one showing enough displacement of the base of the Cambrian rocks to call for a considerable amount of forcible intrusion and the other apparently showing relatively little displacement of the pre-Cambrian rocks. The solution of this dilemma is, I think, that the criteria for forcible injection of intrusive rocks into steep-dipping, schistose rocks are imperfectly established. Simple bodily displacement of wall rocks, such as was pictured for some individual dikes (fig. 1) and postulated by Grout (1941), are very convincing where they can be found, but the absence of such simple relations probably cannot be used as an argument against the mechanism of forcible intrusion. In steeply dipping weak and schistose rocks, lateral and vertical displacements probably are converted in relatively short distances to a general inflation and doming, particularly if the intrusives are built up into composite bodies by successive injections. The map of the relations between intrusives and schist structure (fig. 9) may give a hint of the mechanism by which space for the intrusions has been gained. In the area of schist south of the main intrusive area it was found to be impossible to follow continuous contacts, as can be done in the eastern part of the map; but the distribution of the lithologic types indicates the existence of small wedges of different formations. This pattern suggests the possibility of the existence of small, wedge-shaped fault blocks like those actually mapped in the Cambrian rocks along the northwestern edge of the map. If the schists close to the intrusive bodies are cut up into small wedges by faults which cut the schistosity only at low angles, a mechanism would exist for displacing outward and upward considerable volumes of schist. The field relations have been studied with this possibility in mind, but no evidence either for or against the necessary number of small faults could be established. The "paired" faults already described may, however, be examples of such faults. Those faults cannot be detected with certainty in the schists.

The mechanism of forcible intrusion of large dikes and stocks proposed here must, of course, result in an upward movement of appreciable bodies of rock, in size on the order of that of the total bulk of intrusive material, because ultimately the only direction of escape for the displaced material is upward. Lateral displacement in weak, schistose rocks must be transferred to doming within relatively short distances (Grout, 1945). One who supports the hypothesis of forcible intrusion must be prepared to demonstrate that upward movements of large bodies of rock have taken place. In the case of the Deadwood-Trojan uplift, erosion has removed the evidence of

most of the upward movement, but in the northern Black Hills several stages of erosional dissection of similar uplifts are preserved. A score or more of large and small domes are scattered throughout a zone crossing the broad uplift of the Black Hills. The Deadwood-Trojan uplift, the major part of which is shown in figure 8, is one of the largest of these and shows the deepest erosion; but a still larger uplift, constituting the Bear Lodge Mountains in Wyoming, has a core of Eocene porphyries enclosing segments of pre-Cambrian granite which have been raised 3,000 or 4,000 feet from their normal position. In the Nigger Hill or Tinton uplift, on the line between Wyoming and South Dakota, segments of pre-Cambrian schist have been brought up at least 1,000 feet. A small uplift northeast of Deadwood shows a small area of pre-Cambrian schist several hundred feet above its normal position. In each of these uplifts, Eocene intrusive rocks occupy central positions in the uplifts and are the obvious causes of the doming.

In contrast to these four uplifts in which pre-Cambrian rocks are exposed, the other uplifts of the northern Black Hills disclose more recent rocks, Paleozoic and Mesozoic in age. These uplifts, in which only Paleozoic and Mesozoic strata are exposed, have commonly been described as laccoliths, but it seems more probable that they overlie domes like the Deadwood-Trojan dome. Even some of the domes in which pre-Cambrian rocks are exposed have been called "laccoliths" (Darton, 1904; Smith, 1905; Ferguson and Turgeon, 1908), and the pre-Cambrian rocks were thought to have been floated up in the Tertiary magma; but the field relations hardly permit such an interpretation, and it seems doubtful whether the same authors would argue for that mechanism today.

In using the term "laccolith," some definition is necessary. As first used by Gilbert (1877), the term was defined broadly, but the examples chosen for illustration were more limited in application. The broad definition included all igneous rocks intruded into sedimentary rocks which, by the force of intrusion, lifted the overlying rocks. This definition would apply to most forcibly injected intrusive bodies and to them all if we use, in place of "sedimentary rocks," merely "earlier rocks," and if we add that lateral thrusting-aside is a step in the upward movement of the rocks. Most American geologists have chosen, however, to use the term in the more restricted sense of the examples pictured in the Henry Mountains, in which the sedimentary rocks are in nearly horizontal position and the intrusive bodies were thought to have plano-convex shapes and narrow feeding channels. Many European geologists use the term in the broader sense, and some American geologists do the same, notably, in this connection, Paige (1925), in his discussion of the structure of the Black Hills. The question raised here is whether any of the domes and accompanying intrusive bodies in the northern Black Hills even remotely resemble Gilbert's examples of laccoliths from the Henry Mountains or whether they may not be the effects of forcible intrusion of stocks into the subjacent schists. It is obvious in some cases that intrusions of sills have added to the effects of doming; but, where the pre-Cambrian rocks have been domed, it is clear that the generally accepted laccolithic mechanism was not the cause of the doming.

Some of the smaller domes in Paleozoic and Mesozoic rocks are almost per-

fectly symmetrical, and, whether they measure deformation by laccoliths or by injected stocks, they give an opportunity to study the way in which domed rocks accommodate themselves to the forces causing the doming. Three structures were studied in some detail and were selected for their size and symmetry and for the fact that the Permian(?) Minnekahta limestone, a thin-bedded but resistant rock, gave nearly continuous outcrop surfaces. Figure 10 shows the results of the study of fracture patterns in these structures, in two of which the Minnekahta limestone crops out in a circle around the domes and in the third of which the limestone forms the crest of the dome. By deductive reasoning it might be concluded either that the rocks over domes like these would show radial fractures caused by tension or that they would show a network of circumferential fractures caused by the upward shearing of a circular plug. The observed fracture patterns show neither of these effects clearly, however, although there is a suggestion of a circumferential pattern on Green Mountain (fig. 10, *C*, *D*). On the Elkhorn Peak dome (fig. 10, *A*, *B*) many of the fractures dip inward, and the angular value of the dip of each fracture is in many cases about complementary to the dip of the strata. This relationship can be interpreted to mean that the observed fractures antedated the doming and were deformed by the doming. Possibly they are part of a regional pattern. It must be emphasized that the question we are interested in here is not the cause of the doming but the effect of the doming on the roof rocks. The rocks are obviously domed. The Minnekahta limestone dips as steeply as 54° on the south side of Elkhorn Mountain and as steeply as 25° on the west side of Green Mountain and also on Lime Butte. Cross sections of these domes would seem to indicate a measurable stretching of the rocks, quite beyond anything attributable to the fracturing, even if that is considered to be a result of the doming. But stretching of the rocks over any dome may be only apparent, not real; in fact, it must be only apparent if the doming is a result of pure vertical shearing. In true shear folding, stratigraphic thickness would be measured in the direction of shearing, and there would be no crustal shortening or thinning and thickening of beds. In these three examples of domes caused by the forcible intrusion of magma (whether as stocks or as laccoliths), the roof rocks were able to resist collapse and even fail to show any fracturing or tensional failure that can be clearly ascribed to the doming itself. The surfaces of movement on which the doming was effected are probably as widely distributed and as small as the intergranular boundaries or even crystal cleavage planes.

FORCIBLE INTRUSION BY BATHOLITHS

The examples chosen for study of the forcible intrusion by small and large dikes and stocks are all in the northern Black Hills, but for a study of the forcible intrusion by batholiths it is necessary to go farther away. A suitable area for this purpose is that of the Sierra Nevada of California, which has been studied in recent years by Cloos, Mayo, and others. The rest of this paper will be, therefore, a discussion of the evidence for forcible intrusion by the Sierra Nevada batholith and a discussion of some of the arguments for or against forcible intrusion which have been advanced elsewhere.

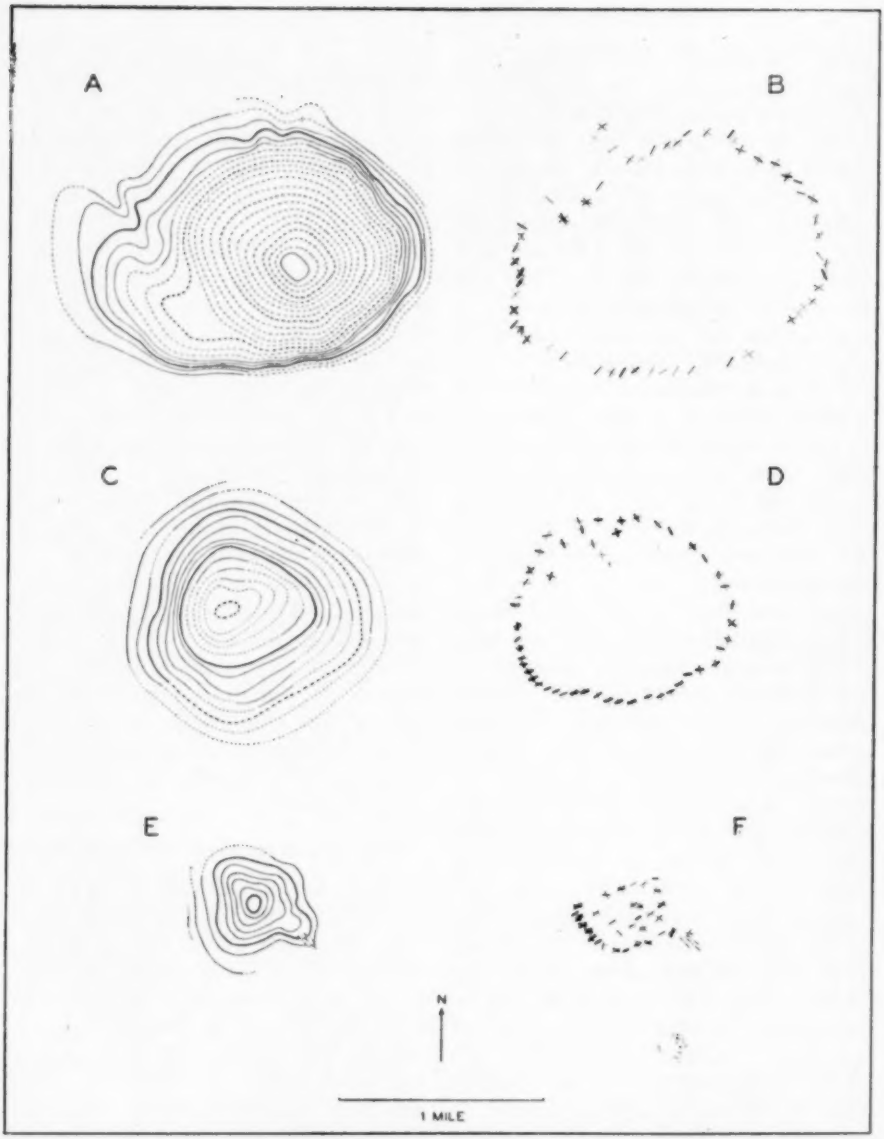

FIG. 10.—Deformation of the Permian (?) Minnekahta limestone over buried laccoliths or stocks in the Black Hills. *A* shows structure contours on the top of the Minnekahta limestone, and *B* shows fracture patterns in the limestone, on Elkhorn Peak. *C* and *D* show the same on Green Mountain, and *E* and *F* the same on Lime Butte. The relative strength of fractures is shown by the relative weight of the strike symbols. Solid contours are observed, dashed contours inferred. The contour interval is 50 feet. The dips of the strata range up to 54° on Elkhorn Peak and up to 25° on Green Mountain and on Lime Butte. The dips of the fractures where observed are mostly inward, and on Elkhorn Peak they are roughly complementary to the dip of the strata.

It is not intended to argue that, because it can be shown that small dikes are forcibly injected, it must follow that large dikes and stocks and, finally, batholiths were introduced by the same mechanism. Rather, it is proposed to study the mechanism of intrusion for each of these arbitrarily designated sizes of intrusive bodies, to see what they have in common and what influence differences of size may have had. Probably few geologists will question that small dikes, like those described in the first section of this paper, have been forcibly intruded under certain conditions. On the other hand, it seems probable, if published discussions give an adequate cross section of opinion, that relatively few geologists are willing to grant that large batholiths like the Sierra Nevada batholith have been emplaced principally by forcible intrusion, and many geologists seem loath to accept that mechanism for batholithic intrusion. The size of the intrusive body is, apparently, held to be an important factor in determining the mechanism of emplacement.

The importance of the factor of size of an intrusive body on the mechanism of emplacement is reduced, however, in a given example if it is shown that the batholith is made up of a great number of small intrusive bodies. The accumulated studies of the Sierra Nevada batholith, especially as presented in the recent paper by Mayo (1941), suggest that the Sierra Nevada batholith, 250 miles long and 75 miles wide, was built up by successive intrusions of a great many small bodies of magma. The great abundance of parting screens of schist within the granitic mass, connected in many places by trains of small, disconnected fragments, are analogous in every way to the parting screens of schist which separate the intrusive units in multiple dikes and stocks. Figure 11 is, therefore, offered as the author's interpretation of the tectonic map in Mayo's paper, based on the assumption that all the schist inclusions are remnants of parting screens and that flow layers in the intrusive bodies near contacts are parallel to the contacts. Figure 11 is no more than a suggested interpretation; obviously, it cannot be more than an approximation to the actual shapes of the separate intrusive bodies. Some internal contacts and some schist inclusions near contacts were not used, and there are probably more small intrusive bodies than are shown. Moreover, a detailed study of the internal structures of some of the larger intrusive bodies might suggest that they are made up of separate intrusions. A better interpretation could be made on the scale of the field maps, and without doubt those men who have done the actual field work could greatly improve this interpretation. Starting from the basic assumption that the areas of schist are parting screens, there seems to be no reason to doubt the conclusion, however, that the batholith was built up by successive intrusions of small bodies of magma. The largest single unit in the area of figure 11, for example, is about 10 miles wide and 30 miles long.

Although we may have, therefore, considerably reduced the amount of schist to be displaced by any single intrusive body, if the magma was forcibly intruded, the amount is, nevertheless, not negligible. There is some evidence of the thrusting-aside of the schists by intrusive bodies, more, on the whole, than was shown for the schists of the northern Black Hills, on this map and also on several of the

Sierra Nevada folios (Turner, 1894, 1896; Lindgren, 1896, 1900; Turner and Ransome, 1897, 1898). The indicated displacement is probably not enough, however, to account for the total volume of intruded rocks; and, if the magma was forcibly intruded, it seems probable that the main displacement of the schists was upward and that a dome of highly deformed schists was built up by the successive injections of magma. There is no indication in the petrology of the intrusive rocks, however, that the confining cover was completely breached during the main intrusive period.

Additional evidence bearing on the mode of emplacement of the Sierra Nevada intrusive bodies is found in the internal structures of the bodies—a line of evidence not available in the Black Hills intrusive bodies, which are almost

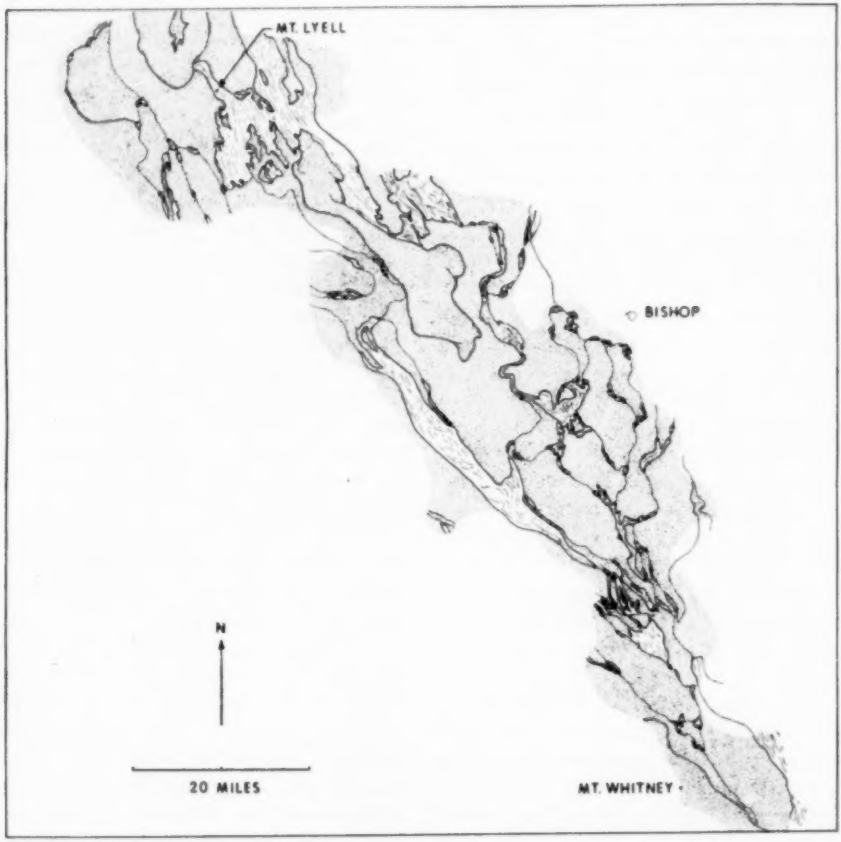

FIG. 11.—Interpretation of a tectonic map of a part of the Sierra Nevada (modified from Mayo, 1941). Areas of intrusive bodies are shown by stippling. Contacts against schist or against screens of schist are hachured; those inferred from flow lines in intrusive bodies by single lines.

wholly devoid of internal structures. The results of the studies by Cloos (1931, 1932, 1933) can mean only that many of the intrusive units of the Sierra Nevada batholith were formed by bodies of magma which expanded upward and to some extent outward throughout their liquid stage and even during part of the solid stage. Balk (1937) has shown that internal igneous structures of this sort are difficult to interpret except as evidence of forcible intrusion of the magma, and he has listed some of the difficulties of detecting forcible displacement merely by a study of the wall rocks.

If the structure of the Sierra Nevada batholith (as shown in fig. 11) is compared with that of the Deadwood-Trojan area (fig. 8) and the dike swarm in the Homestake mine (fig. 3), one common feature will be seen: each intrusive mass has been built up by successive injections of magma into a compact area, the later intrusions being guided by the earlier ones. In each area, narrow parting screens of highly deformed schist separate the individual intrusive bodies. The narrow dikes (fig. 3) are the most regular in shape; the larger dikes and stocks (fig. 8) somewhat less regular; and the large units of the batholith (fig. 11) the least regular of all. These differences may indicate that the larger intrusive bodies, because of slower cooling, were susceptible to deformation by later intrusions, whereas the smaller bodies, by more rapid cooling, were already rigid and strong when later injections arrived.

APPRAISAL OF THE EVIDENCE PERTAINING TO FORCIBLE INTRUSION

In appraising the evidence in support of the concept that the intrusive bodies in the Homestake mine and the Black Hills were forcibly injected, we find that the most compelling evidence is that of the doming of the nearly horizontal Paleozoic strata, and it should be noted that this evidence is available only because the Paleozoic strata have been only partly removed by erosion. Additional evidence of forcible intrusion is given by displacements of the base of the Cambrian strata in the dike zone in the Homestake open cut and in the "paired" faults in natural exposures around the periphery of the dome. This additional evidence again is available only because of the incomplete removal of the Paleozoic strata. Where the erosion of a dome has not yet disclosed the pre-Cambrian rocks, no amount of study of the surface can distinguish between the laccolithic mechanism (of the Henry Mountains type sections) and the forcible injection of an intrusive stock of the approximate size of the dome. If erosion has completely removed the nearly horizontal cover, on the other hand, leaving only steeply dipping, schistose rocks surrounding the intrusive bodies, it would seem to be exceedingly difficult, on the basis of experience in the Black Hills, to measure the amount of displacement of the schists. Only in the exceptionally good exposures in the Homestake mine is there clear evidence of forcible displacement of the schists in amounts equal to the volumes of the intrusive bodies. If only the surface were available for study and the Paleozoic rocks had been completely removed by erosion, the visible displacement of the schists would seem to be quite inadequate to explain the emplacement of the intrusive bodies wholly by forcible injection.

Not only would it be difficult, in the absence of evidence of the doming of the Paleozoic rocks, to prove large-scale

forcible injection of the intrusive bodies in the Black Hills area, but, in addition, some of the relations between intrusive bodies and schist would seem to be opposed to that conclusion. For example, discordant contacts between intrusive bodies and schist, as shown by figure 9, are not uncommon, and, if the areas of intrusive bodies are removed from the map, the resulting voids cannot be filled by bending the wall-rock schists back into place. To reconcile the evidence for forcible intrusion shown by the Paleozoic rocks with the lack of evidence for it shown by the schists, it must be concluded that large amounts of schist were displaced upward and that discordant contacts and complete removal of schist from the map area are not incompatible with large-scale forcible injection. The missing volumes were carried upward and were removed by erosion before the intrusive bodies were exposed to view. Surface exposures rarely permit the tracing of the schist contacts directly against the intrusive bodies, and probably the relationship is generally that previously described and shown diagrammatically in figure 6, *B*.

It must be concluded, therefore, that, where intrusive bodies are surrounded by steep-dipping schistose rocks, discordant contacts and apparent disappearance of even large volumes of wall rocks are not valid arguments against forcible injection of magma unless it can be shown that the disappearance of the wall rocks is real in all three dimensions. It is, of course, rarely possible to do this over large areas. Moreover, even where maps show apparent disappearance of large volumes of flat-lying strata, the third dimension is needed before the injection mechanism can be ruled out, as punched-up blocks of flat-lying strata—the so-called "trap doors" of Knechtel (1944)—are not uncommon structures.

It is the belief of the writer that tacit acceptance of the mechanism of assimilation or magmatic stoping by the magmas of batholithic intrusions has at times made us disregard or underestimate the importance of some lines of evidence favoring the forcible intrusion of the magmas. This conclusion and the discussion of specific arguments which follows are not meant to be a criticism of recent papers on the Sierra Nevada batholith but are merely a general discussion of arguments stated or implied in widely scattered papers. For example, the occurrence of "roof pendants" is frequently mentioned in descriptions of batholiths, but the question at once arises whether these are not inverted ridges and wall-rock screens between separate intrusive units. There are even references to "roof pendants" between intrusive bodies of distinct lithologic character. Ridges and wall-rock screens between separate intrusive units are not proof of forcible intrusion of the magma, but, by analogy to the Black Hills occurrences, they are more easily explained by injection than by assimilation.

Not infrequently the presence of "inclusions" of rock with attitudes parallel to those of the wall rocks is cited as proof of granitization or replacement. The same relationship, however, quite frequently results from multiple intrusion, where parting screens are left between separate intrusions, because the screens, though displaced laterally by the width of an intrusive unit, generally preserve their orientation. In figure 3 the pre-Cambrian structure in the screens is parallel to that of the walls, and in figure 11 the same is true between

adjacent "inclusions," here interpreted as screens. Narrow, sinuous "inclusions," by their very nature, are more likely to be screens than inclusions, especially if the rocks are schistose and weak.

In geologic literature there is rather widespread use of the concept of a "buttress" of an intrusive stock or batholith against which folded rocks have been crumpled, but the examples should, in the writer's opinion, be scrutinized with great care. The term, of course, is used to mean an immovable body, which is by no means the sense of the engineering or architectural term, and it is in this sense of an immovable body that the use of the term will be discussed. In the first place, it is obvious that the pattern of flow lines set up in a viscous mass pushed against an immovable rigid body would be identical with the flow lines set up if the rigid body were to be pushed a short distance into the viscous mass. Moreover, if the rigid body were small in area as compared with the area of the viscous mass, identical flow lines would result, whether the viscous mass moved slowly past the immovable rigid body or the rigid body moved slowly through the viscous mass; but in either case there would be a piling-up of the viscous material on only one face of the rigid body. If the rigid body were growing in the midst of the viscous medium, on the other hand, there would be an apparent piling-up of the viscous medium on all faces. Under these conditions, a forcibly intruded body would look like a "buttress" when viewed from any direction. In the case of small intrusive bodies—stocks and small batholiths—there is good reason to question the use of the term "buttress," unless it can be shown that the wall rocks have tended to move against one face, around the sides, and away from the opposite face. In the case of very large linear batholiths like the Sierra Nevada batholith, longer than the width of the crustal segment supposedly moving against the "buttress," these criteria are not available, but it still must be remembered that the piling-up of the wall rocks against the intrusive rock measures only relative movement. In the author's opinion, a "buttress" of igneous rock against which schists are supposed to have been crowded is more probably an injected igneous body, although admittedly such "buttresses" may exist. Under favorable conditions the time relations between intrusion and deformation might be significant; for example, if the deformation could be determined to be later than the intrusion, then the intrusion obviously could not be the cause of the deformation and so may have acted as a "buttress."

An objection sometimes raised against the concept of the forcible intrusion of bodies as large as batholiths is the difficulty of conceiving how the rocks in the roof, supposedly under tension by the spreading action, could exert enough resistance to the rising magma to prevent its extrusion at the surface. These objections are answered in part by the deductions from the experiments of Grout (1945) and in part by the evidence from the field studies of the roofs of buried "laccoliths" of the Black Hills previously described (fig. 10). It seems that strong tensional effects are developed only immediately over the intrusive body, and so foundering of the roof would not occur unless the intrusive body rises very close to the surface. Where the intrusive body is rising through deep or intermediate levels, the dominant deformation at shallow levels may be a broad doming produced by shear folding, in which either there is no stretching of the rocks or the tensional effects are offset by dilation.

In many places it has been possible to determine the relative ages of deformation and igneous intrusion, and the fact that intrusion is later than deformation has usually been considered to be an argument against forcible intrusion. Bucher (1933, pp. 274–282), for instance, adopts this conclusion in stating a "law" of geology. The conclusion should, in fact, be just the opposite; the time relations argue in favor of forcible injection. To say that the result invariably follows the cause may be good logic, but it is not good geology, as we geologists somewhat loosely use the terms "cause" and "effect." It should be obvious that the deformational effects caused by the forcible injection of a body of magma precede the body of magma, both in space and, at a given point, in time. Deformational effects in the wall rocks of a compressive character which precede the intrusive bodies can be, and probably are, effects of the forcible injection of the magma. For example, the "paired" faults of the Deadwood-Trojan dome are pre-intrusive but were caused by the intrusion, and the same is true of the small fault at the dike end in figure 1, *B*.

Although the significance of this time relation between intrusion and deformation seems self-evident, only three clear references to it are known to the writer, one by Balk (1937, p. 126), one by De Lury (1941, p. 59), and one by White and Jahns (1950, p. 218).

Deformational effects which follow the injection of the magma in space or, at a given point, in time will, in general, be tensional in character, except for the minor deformational effects of compressive character which, as Balk (1937, pp. 101–106) has shown, are sometimes carried over from the liquid stage of intrusion into the early part of the solid stage.

Applying these generalizations regarding the appraisal of the evidence of forcible intrusion of magma to the specific area of the Sierra Nevada batholith, it would seem that no valid arguments against forcible injection of the intrusive bodies have been advanced. Mayo (1941, pp. 1070–1073) gives two reasons for discarding, apparently somewhat reluctantly, the theory of a forcible injection of the magma. The first is his inability to conceive how the roof over the batholith could have been stretched for some 70 miles and still have strength enough to resist the force of the rising magma. The answer may be, first, that there was little or no stretching of the roof and, second, that the force of the rising magma was restrained in the end by internal friction. To the extent that the roof was raised by pure shearing, there is, of course, no stretching; and, if shearing is combined with rotation of mineral grains, the schistose rocks may even increase in bulk, that is, undergo dilation (Noble, 1950). This dilation is common in zones of deformation in schistose rocks, and it can probably account for all the apparent stretching indicated by the thrusting-aside of wall rocks around the batholith. That even horizontal, nonschistose rocks may be domed with little or no evidence of tensional fracturing is shown by the descriptions of hidden "laccoliths" in the Black Hills (fig. 10). Mayo's second reason for discarding the theory of forcible injection of the magma was that the magma came after the formation of the rock cleavage, which was developed during the folding, and hence the folding could not be caused by the intrusion because it is earlier than the intrusion. It has already been pointed out, however, that these time relations not only are compatible with the mechanism of forcible in-

jection of magma but are required by that mechanism. In the absence of objections more valid than these, it would seem that there is good reason to ascribe the emplacement of the Sierra Nevada batholith to forcible intrusion.

ACKNOWLEDGMENTS.—I am indebted to the Homestake Mining Company for permission to publish the results of geologic studies in the Homestake mine and the Black Hills. My colleagues, J. P. Buwalda and R. H. Jahns, have read the manuscript critically and offered many suggestions.

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SUBMARINE CANYONS BORDERING CENTRAL AND SOUTHERN CALIFORNIA[1]

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ABSTRACT

The characteristics of California submarine canyons between Monterey and San Diego differ from land canyons, suggesting that their origin is not subaerial: longitudinal profiles are steeper than those of most land canyons; profiles are more irregular; canyons head near or at sea-level but extend to different depths, some greater than 10,000 feet; etc. Geologic data show that the heads of Redondo, Hueneme, Mugu, and Newport canyons cut Upper Pleistocene and Recent sediments and that no land stream has issued into the Redondo Canyon head during the late Pleistocene and Recent. These arguments, with others, suggest that California submarine canyons were cut by submarine processes which are still operating today in some canyons.

Wave-approach and refraction studies along the California coast, with observations, show that considerable sediment brought to the sea by land rivers is moved along the coast by longshore processes. California submarine canyons occur where (1) a supply of sediment is brought to canyon heads by these shore processes, (2) a relatively steep offshore slope heads close to shore, and (3) the transporting power of the longshore current slackens, usually because of coastal configuration. The canyons are probably eroded by the seaward movement downslope under gravity of sediment by some kind of sediment flow, turbidity, density, or suspension current.

INTRODUCTION

Hydrographic surveys from all parts of the world show that submarine canyons are widespread (Shepard, 1948*b*, p. 231; Kuenen, 1950, p. 485) but differ somewhat in essential features from place to place. Today geologists still face the perplexing problem of explaining their formation. Perhaps by focusing our attention on those occurring along the California coast, where a large amount of data has been acquired in recent years, progress can be made toward understanding their origin; however, it does not follow that canyons elsewhere necessarily came about in precisely the same way. In this paper, previous descriptions of California submarine canyons are reviewed and compared with geologic data, on the one hand, and oceanographic and meteorologic information, on the other.

PREVIOUS WORK

Davidson in 1887 was the first to draw attention to California submarine canyons. Ten years later, in 1897, he described most of the conspicuous canyons along the coast and published contoured charts, although he did not comment on their origin. During the interval between these papers, two discussions on canyon origin were published. First, Le Conte (1891) explained them as cut by rivers at the end of the Pliocene. Second, Lawson (1893), after pointing out that submarine canyons along the California coast "are of peculiar interest because many of them end abruptly near the shore and have no corresponding rivers on the coast" (p. 57), concluded that the canyons were tectonic in origin, formed by faulting and folding (see also Lawson, 1924). Fairbanks (1897) disagreed with Lawson and concluded from the existence of alluviated valleys along the coast that, during emergence, rivers had cut to base level now 1,200 to 1,500 feet below sea-level. He was, however, puzzled by the fact that "the valleys beneath the water do not in some cases seem to bear any relation to the topography of the land lying opposite" (p. 230) and "opposite the

[1] Manuscript received December 11, 1950.

mouths of some of the larger streams no submarine valleys have yet been found" (p. 231).

In 1902 Ritter (p. 59) described some dredgings from the Redondo Submarine Canyon which suggested to him "that the valleys are natural channels through which currents flow, at times at least, *from the shore out to deeper water*." He found (p. 59)

> that the bottom deposits of some . . . for example that at Redondo, even at the distance of several miles from shore, are of a character to prove that close inshore material is carried into them in large quantities. Shore-worn shells of strictly littoral, and even fresh-water species; fragments of drift-wood; kelp hold-fasts, of which none grow in the immediate vicinity, etc. were taken in abundance by the dredge.

After reading Ritter's article, W. S. Tangier Smith (1902) made several comments on the origin of the California submarine canyons which I re-emphasize nearly fifty years later. He reached the conclusion (p. 670) that "the majority of the submarine channels of the California coast have been formed, or are at least kept open, by some cause now in operation, and that cause coastal currents." Some of the points which he stressed are: (1) many of the submarine canyons "stop abruptly at or near the shore line" without a corresponding valley on land; (2) the gradient of the submarine canyons is "considerably greater than that of the lower coastal valleys"; (3) where the submarine canyons "are opposite important valleys on the shore, [they] do not correspond to them in size, shape, or [gradient]"; (4) some of the submarine canyons "head opposite the mouths of large valleys on the land, and some opposite abrupt and rugged portions of the coast, where there is no break in the Coast Ranges. . . . [They] do not, in general, correspond in position to valley openings on shore, and *vice versa*, the coast drainage lines (except in a very few instances) have no corresponding submarine channels. . . . Two of the valleys head near the points of cuspate forelands, the valleys, in both instances, running in close to the beach"; (5) "it is possible that many if not most of the valleys are due, not to any one cause, but to several causes which have all contributed to their formation or preservation"; and (6) "each valley must be considered by itself, since the explanation for any one is not necessarily the explanation for all."

William Morris Davis (1934) published a brief discussion of some of the California submarine canyons following a study of the coast of the Santa Monica Mountains. He found (p. 298) "no evidence in the mountains to indicate a recent upheaval and subsidence by any such measure as 1200 or 1800 feet" required by a subaerial origin for the nearby submarine canyons (Hueneme, Mugu, and Dume). Furthermore, in the submerged coast (p. 299)

> there should now be as many submerged valleys as there are good-sized mountain streams, and there are not. Only one submerged valley is found that might possibly be of this origin, and there should be at least ten or twelve. Moreover, the Huemene submerged valley fronts the Oxnard plain, which appears not to have suffered uplift with the mountains but to have long been the site of subsidence.

He noted also (p. 299) that

> ordinary depositional processes, which have unquestionably been acting to build up the shallower sea floor adjoining the submarine valleys, ought to have obliterated the inner parts at least of the existing submarine valleys, unless some marine process of opposite nature and still in operation had kept them empty."

Davis recognized a relation between coastal configuration and the occurrence of the "submarine mock valleys," as he

called the canyons, and inferred that they were eroded by seaward deflections from longshore currents. In regard to Monterey Canyon he (p. 307)

> noted that the form of the bay is admirably adapted to the development of an outgoing bottom current to compensate for an inward drift of the surface waters under the action of severe onshore winds.

In summary he states (p. 308):

> It is only by individual study of many submarine valleys in close connection with the geological history of their coast that their origin can be determined; and when thus determined it is highly probable that they will be found to have many diverse and somewhat complicated origins.

The outstanding student of California submarine canyons is Francis P. Shepard, who, with his associates, has assiduously gathered pertinent observations on their form, distribution, sediments, and wall rocks. Without his data and especially without his monograph, *Submarine Topography Off the California Coast* (Shepard and Emery, 1941), the present paper would have been impossible. Following a detailed presentation of descriptive material, upon which I have here drawn heavily, he has reviewed arguments for and against the causes of canyon-cutting that come to mind. By the elimination, in turn, of diastrophism, suspension or density currents, submarine spring sapping, submarine mud flows and slides, and tsunamis as the main agents, he concludes that the canyons were eroded by streams during emergence of the continental shelf and part of the continental slope (Shepard and Emery, 1941, pp. 122–159). Since, in his view, less serious objections confront subaerial origin, he explains the emergence by withdrawal of oceanic water into great Pleistocene icecaps, coupled with a compensatory isostatic upwarping of the continental margins (1948*b*, pp. 242–249). In this way he believes the sea-level to have, in effect, been about 6,000 feet lower than today. His recent discussion (1948*b*, p. 248) summarizes difficulties facing this explanation for the canyons which, to me, warrant greater emphasis. Even more recently (1949, pp. 1610–1611) he has modified his views somewhat, and proposes that "the canyons were excavated at a relatively remote time and have undergone subsequent modifications in response to glacially-controlled changes in sea level."

Recently Woodford (1951) has critically examined data bearing on California submarine canyons, especially Monterey, and has concluded that at least the upper parts have been formed by submarine processes, probably in late Pleistocene and Recent time. Several of his points are dealt with again in the present paper.

Many general articles on the formation of submarine canyons or dealing with canyons outside California have provided data and ideas which are drawn on here. Chief among these are Daly (1936, 1942), Johnson (1938–1939), Veatch and Smith (1939), Bucher (1940), Kuenen (1947, 1950), and Stetson (1949).

SOME CRITICAL FEATURES OF CALIFORNIA SUBMARINE CANYONS

Since Shepard has described the California submarine canyons in detail (Shepard and Emery, 1941; Shepard, 1948*b*, 1949) it is not necessary to review all their characteristics. Instead, a few critical features are selected which are thought to bear on the origin of those canyons from Monterey Bay to the Mexican border. In following the discussion below, refer to figure 7 of this

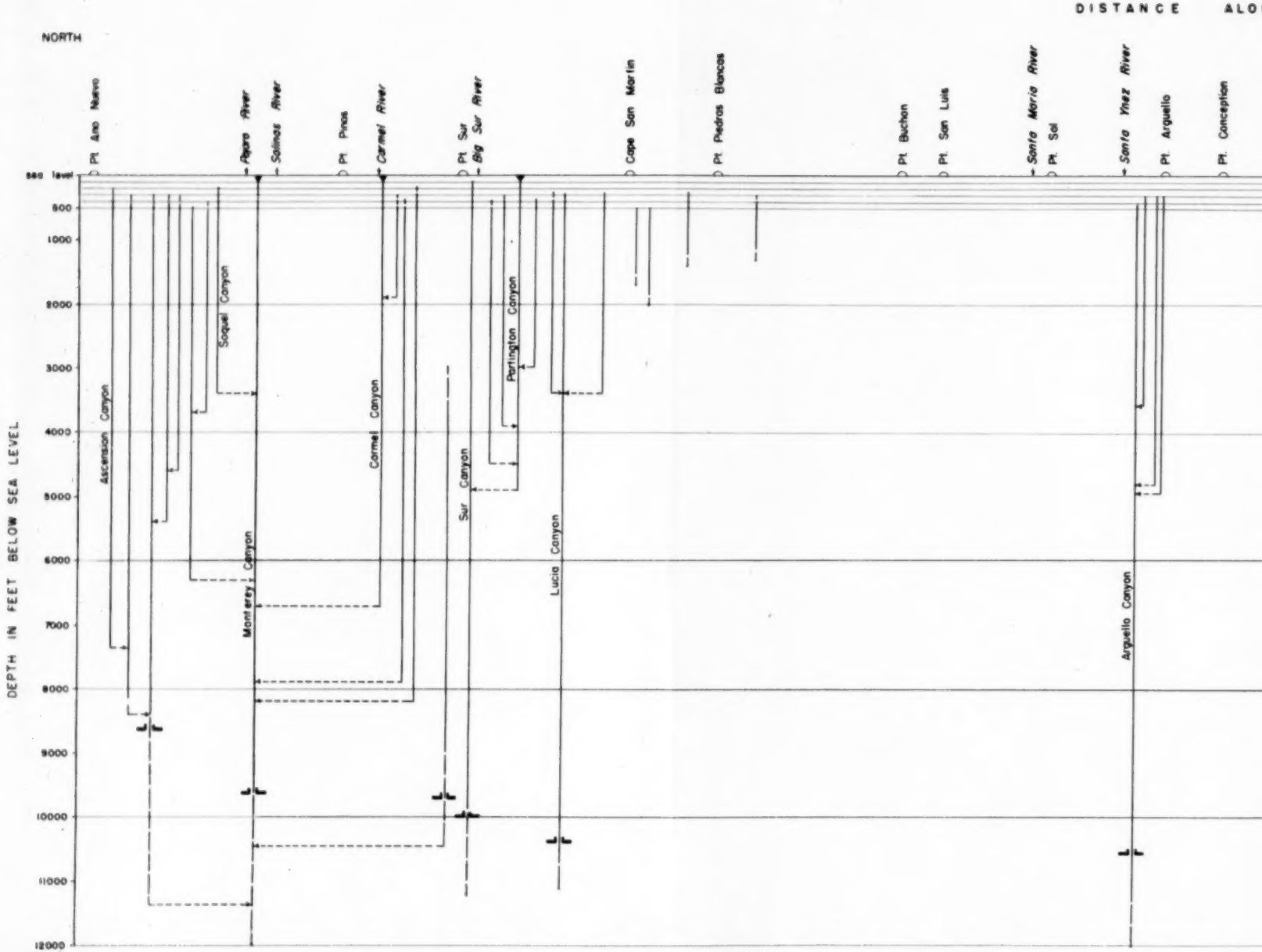
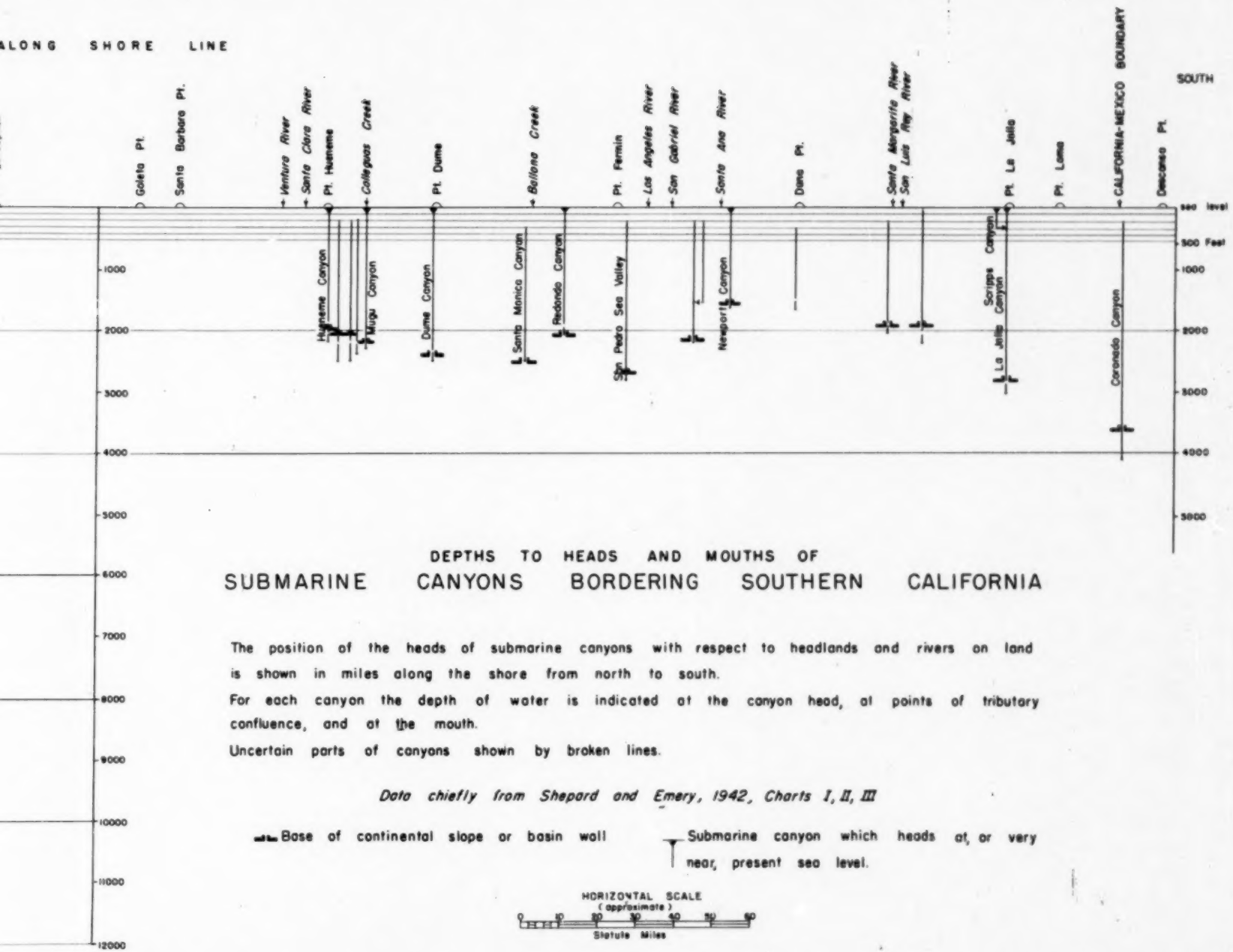


FIG. 1.—Depths to heads and mouths of subma



DEPTHS TO HEADS AND MOUTHS OF

SUBMARINE CANYONS BORDERING SOUTHERN CALIFORNIA

The position of the heads of submarine canyons with respect to headlands and rivers on land is shown in miles along the shore from north to south.

For each canyon the depth of water is indicated at the canyon head, at points of tributary confluence, and at the mouth.

Uncertain parts of canyons shown by broken lines.

Data chiefly from Shepard and Emery, 1942, Charts I, II, III

Base of continental slope or basin wall

Submarine canyon which heads at, or very near, present sea level.

HORIZONTAL SCALE
(approximate)
0 10 20 30 40 50 60
Statute Miles

submarine canyons bordering southern California

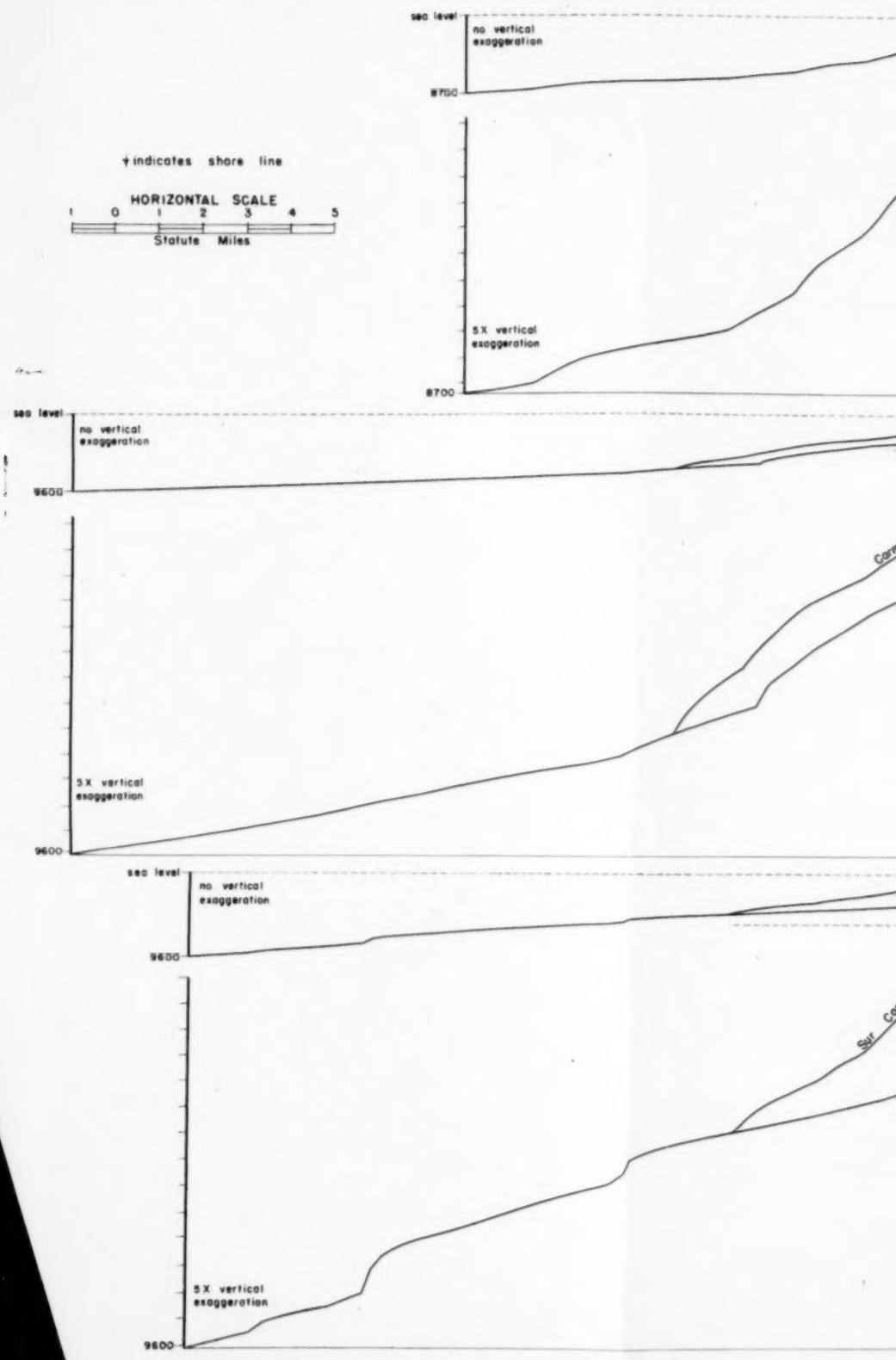
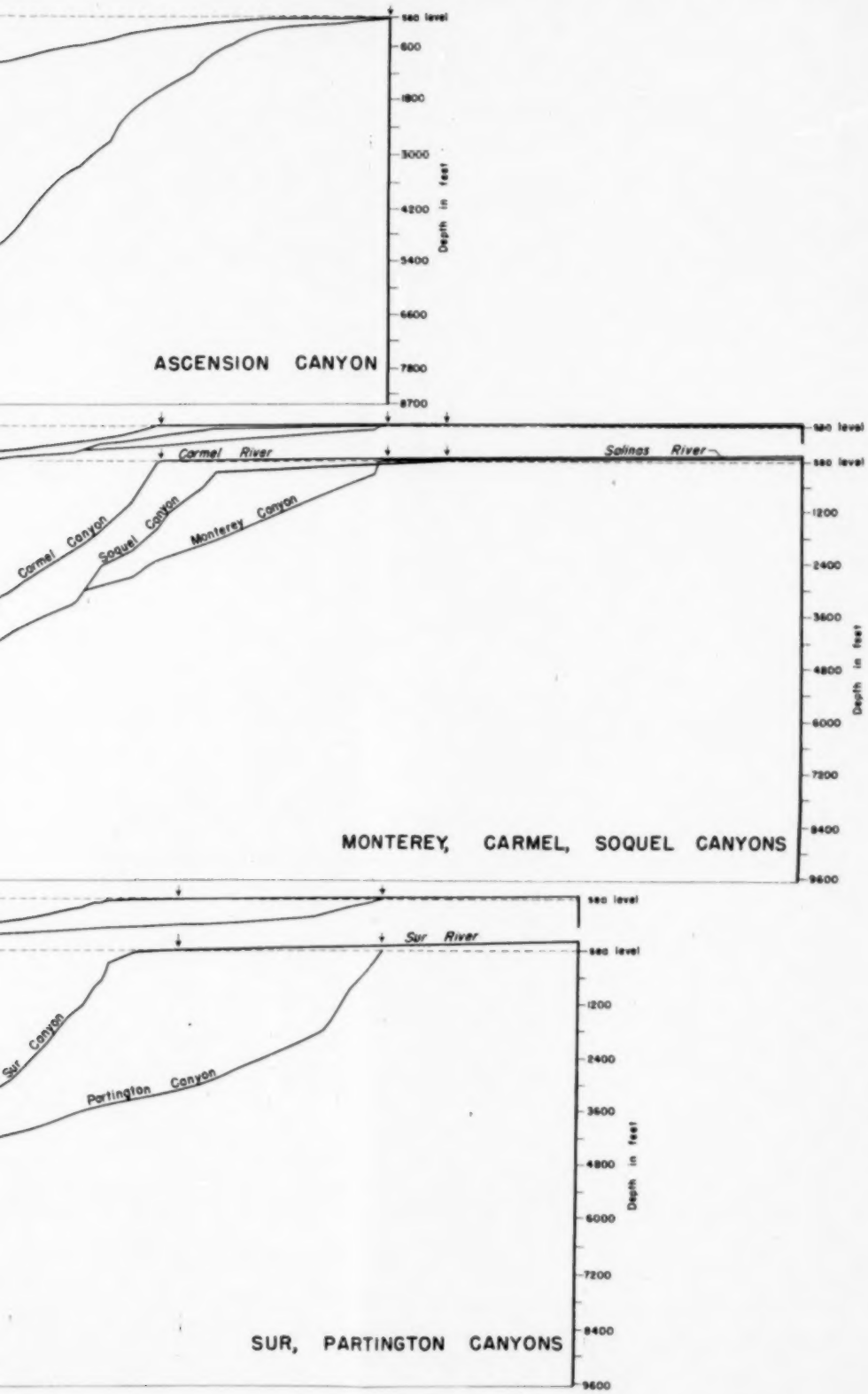


FIG. 2.—Longitudinal profiles of Ascension, Soquel, Monterey, Carmel, Su
U.S. Coast and Geodetic Survey charts and from Shepar



el, Sur, and Partington submarine canyons, California. Data from
hepard and Emery (1941, charts I, II, and III)

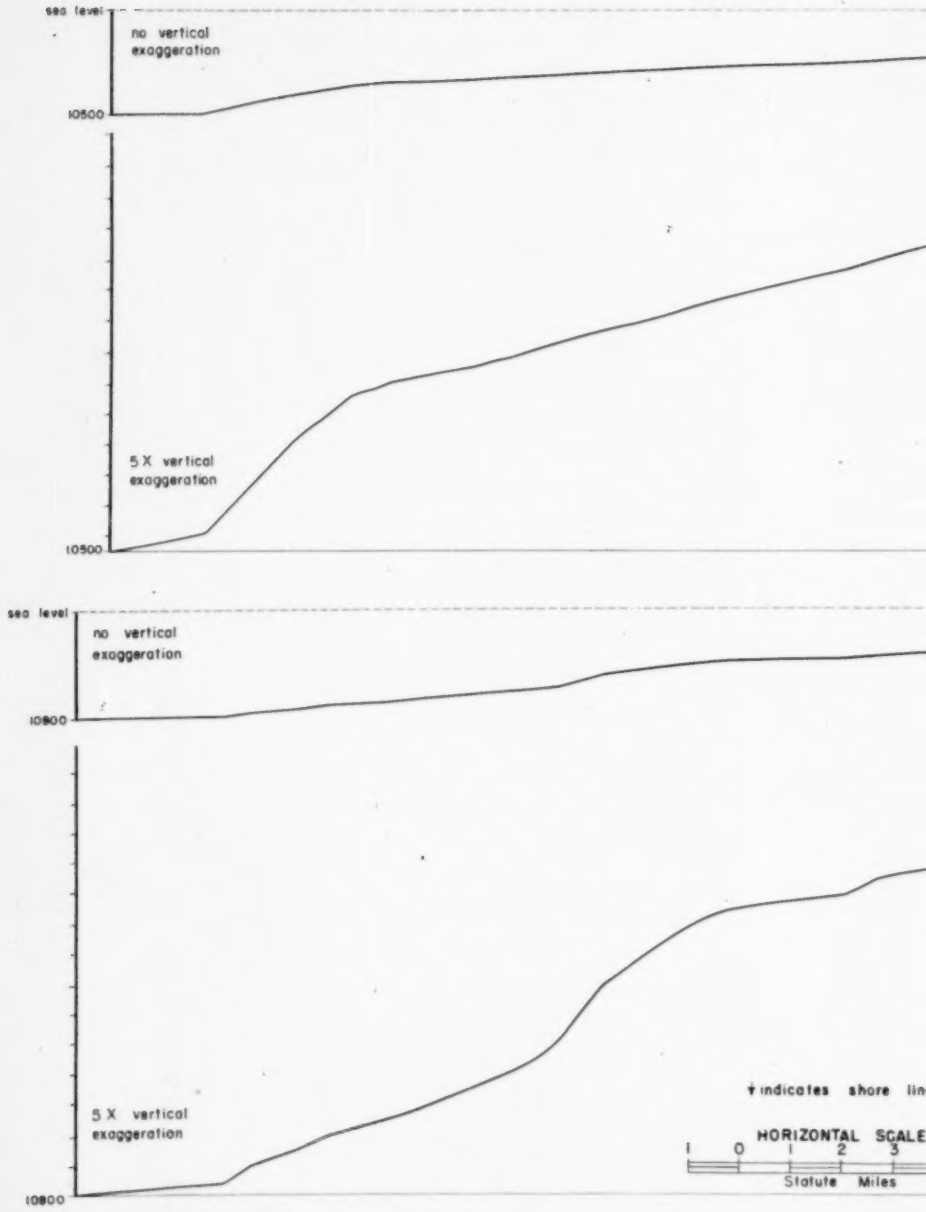
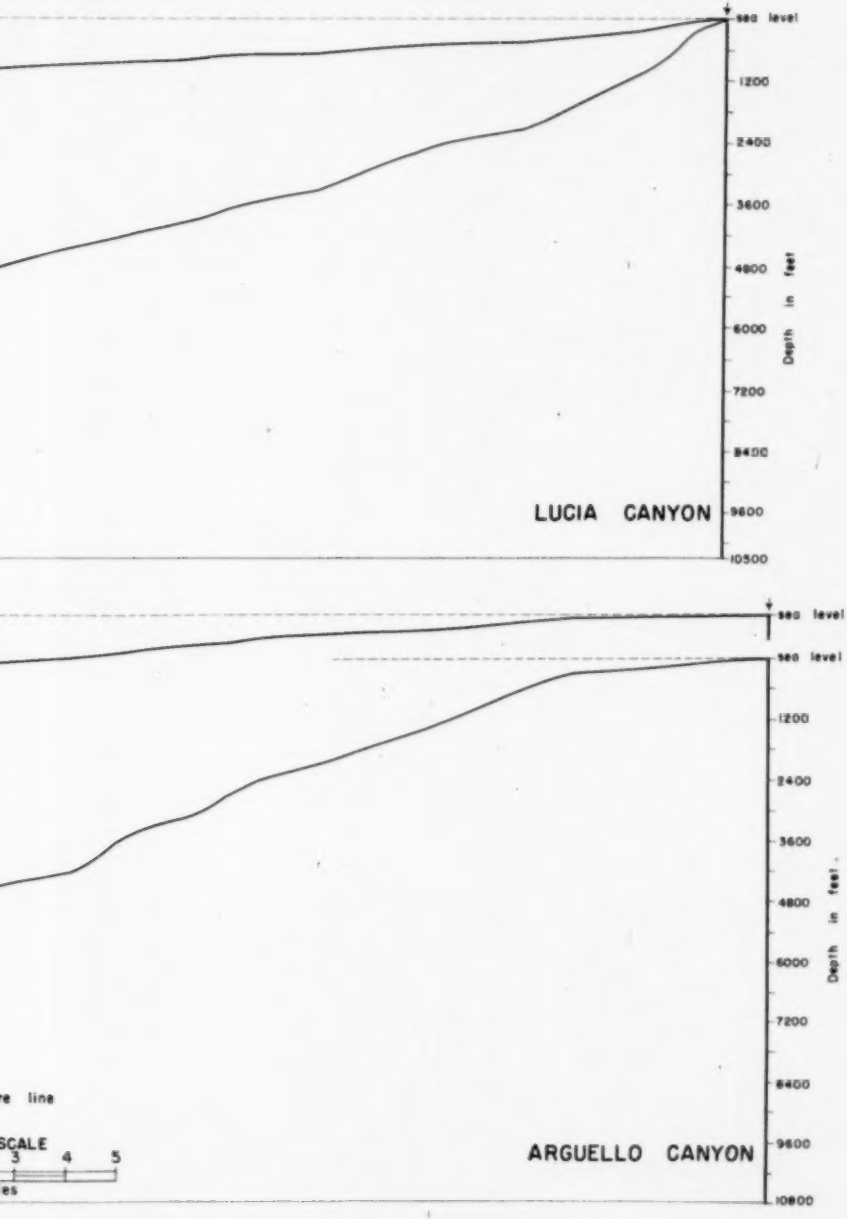


FIG. 3.—Longitudinal profiles of Lucia and Arguello submarine canyons, California. Data from U.S. Coast



Coast and Geodetic Survey charts and from Shepard and Emery (1941, charts, I, II, and III)

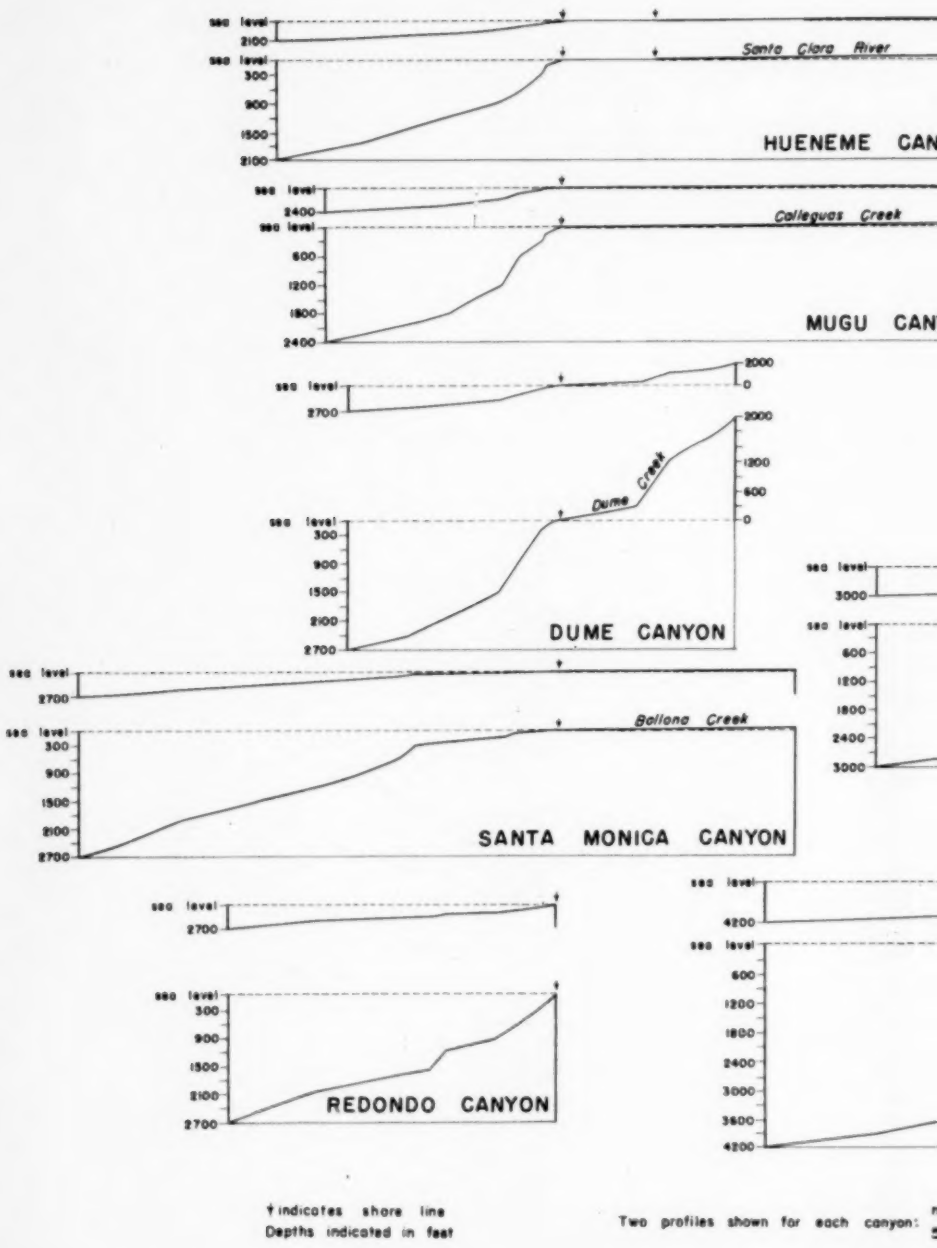
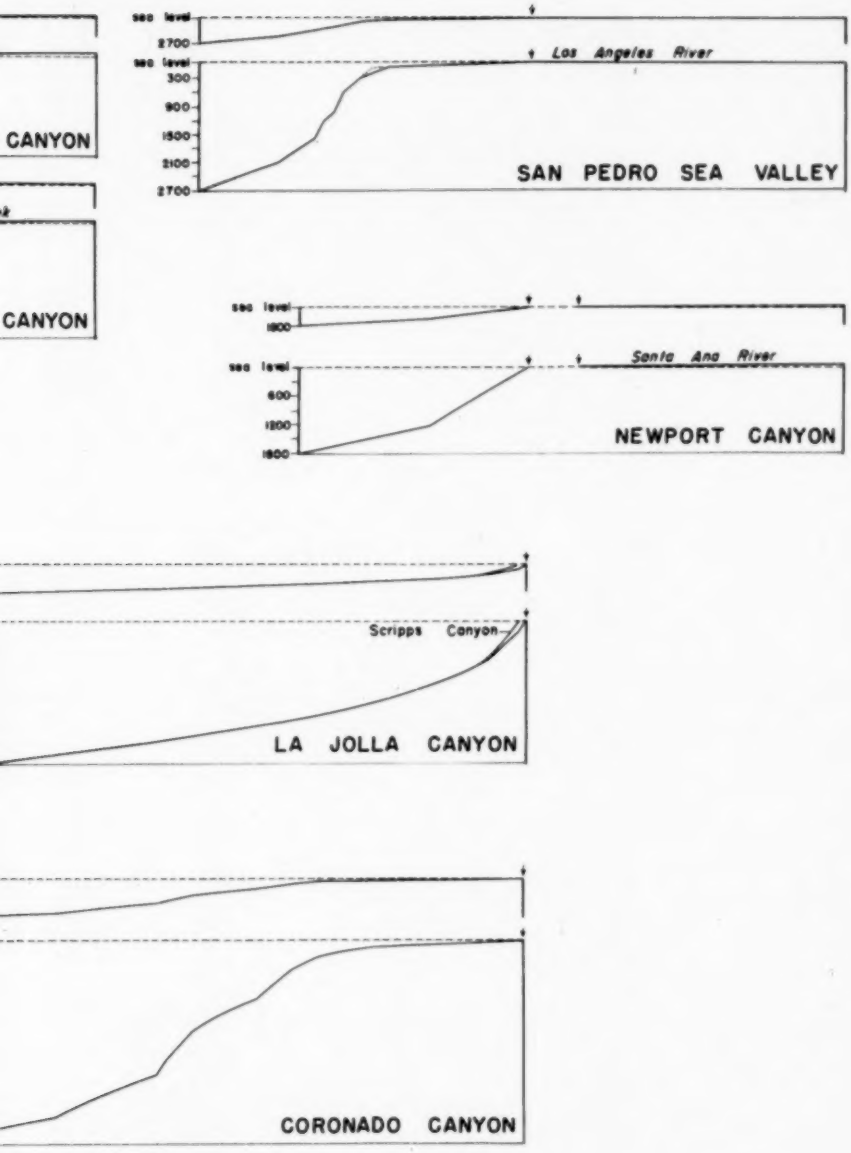


FIG. 4.—Longitudinal profiles of Hueneme, Mugu, Dume, Santa Monica, Redondo, Newport, Valley, California. Data from U.S. Coast and Geodetic Survey charts and f



port, Scripps, La Jolla, and Coronado submarine canyons, and San Pedro Sea
and from Shepard and Emery (1941, charts I, II, and III)

paper as well as to Shepard and Emery's charts I, II, and III (1941).

DISTRIBUTION OF SUBMARINE CANYONS AND THEIR RELATION TO LAND TOPOGRAPHY

More than twenty different submarine canyon systems incise the continental border between Monterey Bay and Mexico. The canyons extend seaward, roughly normal to the coast, from near shore to deep water in basins or at the continental margin. No striking relation exists between present rivers and submarine canyons, although several canyons head near, but not at, the mouths of rivers. Conspicuous among these are Monterey, Sur, Arguello, Hueneme, Mugu, and Santa Monica canyons. Several other canyons, however, such as Partington, Lucia, Dume, Redondo, and La Jolla head against the coast where there are no large rivers. A few, such as Tanner and Santa Cruz canyons and the San Clemente "rift valley," are found offshore or near islands. Since in the present paper an explanation is sought for *each* submarine canyon in the light of its history, this distribution clearly raises questions which will require attention.

DEPTHS TO HEADS AND MOUTHS

For each of the principal canyons and tributaries, figure 1 shows diagrammatically the depth of water at the head and at the mouth, as well as the position of the head in reference to headlands and rivers on land. The abscissa represents miles along the shore line and the ordinate depth below sea-level. Poorly defined parts of canyons, due mainly to a lack of soundings, are shown with broken lines, and the depth of confluence of tributaries with horizontal dashed lines. In several cases the selection of a tributary as the main canyon has been arbitrary.

Figure 1, together with figure 7 and Shepard and Emery's charts, brings out several striking features: (1) All the canyons except one (unnamed, between Monterey and Sur canyons) head within 500 feet of present sea-level, and at least 10 (marked on fig. 1 with black triangles) head within a few feet of the shore line. (2) Few rivers issue directly into canyon heads at the present time. (3) The mouths of the submarine canyons debouch at different levels. Two groups can be recognized: (*a*) Those north of Point Arguello, which reach down to depths below 11,000 feet. These canyons (Ascension, Monterey, Sur, Lucia, and Arguello) issue into deep troughs or reentrants in the continental slope. Although details of shape near their mouths are obscure, Shepard and Emery's charts strongly suggest that they reach to where the abrupt slope meets the undulating hills surrounding the ocean basins. (*b*) Those south of Point Arguello, which reach depths between 1,500 and 4,000 feet. These canyons, from Hueneme to Coronado, extend into troughs or closed basins and can be traced down to the break in slope between the basin walls and the gentle hills rimming the basin floors. In brief, it appears that the submarine canyons extend to depths determined by the subsea elevations of the basins into which they issue.

LONGITUDINAL PROFILES

Examination of the longitudinal profiles of California submarine canyons, reproduced here in figures 2, 3, and 4, reveals that in steepness and in form they differ from the profiles of land rivers. (For other profiles see Shepard and Emery, 1941, pl. 12; Kuenen, 1947, fig. 1; Upson, 1949, fig. 3; Woodford, 1951, pl. 1). These differences have been empha-

sized by Woodford (1951), especially in reference to the Monterey Submarine Canyon. The significance of the nick point near present sea-level, which stands out conspicuously on figures 2 and 4, where the profiles of near-by rivers are shown in reference to profiles of submarine canyons, is discussed in a later section.

With respect to steepness, the profiles are steeper than the profiles of rivers of comparable size on land. Hueneme Submarine Canyon, for example, has an average gradient of about 175 feet per mile over its whole course (12.6 miles), from sea-level down to 2,200 feet, whereas the Santa Clara River has an average gradient of about 12 feet per mile over the same distance up from its mouth. The Santa Clara River, 85 miles long, drops 6,000 feet from its headwaters in the San Gabriel Mountains to give it an average gradient of about 70 feet per mile. Hueneme Canyon is therefore about fourteen times steeper than the lower 12.6 miles of the Santa Clara River, and about two and a half times steeper than the river as a whole. These figures are approximate. Some other average gradients are tabulated in table 1.

In view of the irregularities of the canyon profiles, these average gradients have questionable significance, although they do show roughly the average steepness. Dume (or Zuma) Creek, which enters the sea a short distance west of the head of Dume Canyon, has an average gradient of about 230 feet per mile over 5.4 miles of its course up to its source at 2,000 feet in the Santa Monica Mountains. Carmel is one of the steepest submarine canyons and has about the same steepness as Bright Angel Creek (415 feet per mile). The latter, a tributary of the Colorado River in the Grand Canyon, falls 5,850 feet from an elevation of 8,000 feet at the rim of the Kaibab Plateau to 2,400 feet, 13.5 miles downstream at its confluence with the Colorado River. Only very rough terrains in southern California have streams with gradients comparable to Carmel Submarine Canyon. The east fork of the San Gabriel River, for example, which heads on the slopes of Mount San Antonio (Old Baldy) at an elevation of about 8,000 feet, has an average gradient of about 430 feet per mile over the first 16 miles. In brief, these examples suffice to show that submarine canyons have very steep gradients, usually steeper than canyons of similar size on land.

TABLE 1

Canyon	Canyon Length (Miles)	Depth at Mouth (Feet)	Average Gradient (Feet per Mile)
Monterey	130	12,000	93
Carmel	16	6,700	420
Dume	7.3	2,400	330
La Jolla	13	2,900	140
Partington-Sur	51	9,900	190

As to form, the submarine canyon profiles are largely irregular (figs. 2, 3, and 4) and are not graded like those of land streams. Woodford (1951) has compared the profile of Monterey Submarine Canyon with those of land rivers and has found (p. 833) that the "gradient of the sea valley is much steeper and does not resemble closely any logarithmic curve" such as can be recognized for land rivers, including the Salinas, which flows into Monterey Bay. Down to a point 20 miles from the head, at a depth of about 3,800 feet, a straight line fits the Monterey profile most satisfactorily. Profiles for other submarine canyons demonstrate that straight lines apply to parts of many canyons, although in the main they are too irregular for curve-fitting. Canyons

that head nearly at the shore line have steeper profiles at their heads, which flatten out seaward, a feature they hold in common with river canyons on land. Prominent nicks are characteristic of most canyon profiles, although their depth and size do not appear to correlate from profile to profile. Data in general are too scant to relate nick size and position to topography or bedrock resistance, although Partington-Sur Canyon, for example, has a conspicuous nick in its course where it issues from the continental slope.

TRANSVERSE PROFILES

Shepard and Emery (1941, figs. 20, 21, 24, 28, 29, 30) have drawn many transverse profiles of California submarine canyons. In the main they show relatively steep and irregular walls extending down from the surface of the continental shelf and slope to the flat canyon bottoms. California submarine canyons, therefore, have cross sections like youthful canyons on land with a narrow alluvial plain in the bottom. Since the V-shaped profile on land is principally caused by mass-wasting of walls as a stream cuts downward, the similar profiles of the submarine canyons suggest that mass-wasting may also be an important process in their formation (Kuenen, 1947, p. 53). If so, irregularities and differences in steepness may well reflect differences in wall-rock character.

PATTERN

Shepard and Emery's charts (1941) suggest that the submarine canyon systems have sinuous courses much like land canyons. They differ, however, in that most have fewer tributaries than present river systems of comparable size on land. Furthermore, if present, the tributaries are grouped around the head of the canyon, a feature which contrasts with that of the long lower reaches of the canyons without significant tributaries.

DEGREE OF DISSECTION

Study of Shepard and Emery's charts, in part reproduced here as figure 7, brings out a marked difference in the number of submarine canyons compared with the number of canyons just inland from the shore line. For the 50-mile stretch of coast between Point Hueneme and Santa Monica, Davis (1934, p. 299) has pointed out that ten or twelve good-sized mountain streams are without offshore submarine canyons. This applies not only to the part of the coast discussed by Davis but to the rest of the area under consideration here as well.

In order to bring out this contrast in the degree of erosional dissection between land and the sea floor, the margins of the submarine canyons have been delineated on figure 7, and the area occupied by the canyon outlined by a red dotted line. It is stressed that the demarcation of the canyon area is approximate for several reasons. In the first place, the data used for Shepard and Emery's charts, as well as the contouring, may not be accurate enough to warrant so detailed an interpretation. Second, assuming the contoured charts to be an accurate depiction of the submarine topography, it is not everywhere possible to determine precisely the contact between the undissected submarine slope and the brink of the canyon. With allowance for errors arising from either of these causes, it is still clear that the submarine canyons are geomorphically "young" features and have barely succeeded in notching below the "initial" surface.

The volume of the submarine canyons, as measured beneath an imaginary extension of the adjacent undissected sub-

marine surface across the canyons, is a very small fraction of the total bulk of the continental margin. This feature contrasts markedly with the state of erosion on land. On the coastal mountains, remnants of erosion surfaces surviving from previous geomorphic cycles are common, but their total area is trivial compared to that of the dissected terrain between them. Most of the land canyons near shore are separated by sharp-crested ridges, showing that widespread reduction of the whole land surface is now proceeding. The volume of the canyons on land, then, measured below a reasonable initial surface, is relatively much greater than the volume of submarine canyons.

GEOLOGY IN THE VICINITY OF SOME SUBMARINE CANYONS

Many kinds of rocks representing various ages are exposed near the heads of submarine canyons on shore and on the islands off the coast and have been recovered from the sea floor (Revelle and Shepard, 1939, pp. 267–268; Shepard and Emery, 1941, pp. 86–89; Emery and Shepard, 1945). The belief seems warranted, therefore, that the California canyons have been cut in rocks similar to those exposed along the coast and on the islands. Many of the submarine canyons, such as Carmel, Partington, Dume, and La Jolla, head at the shore line in rocks of mid-Tertiary age or older (Jenkins, 1938). Others, such as Monterey, Hueneme, Mugu, and Redondo, head in younger rocks. These canyons and the geology near their heads are singled out for attention because it is important to determine the age of the *youngest* rocks cut by the submarine canyons in order to narrow down the time interval during which these canyons were formed. The head of Monterey Canyon, for example, has been found by Woodford (1951) to truncate late Pleistocene and Recent sediments. This evidence, supplemented by similar data from Redondo, Hueneme, and Mugu canyons, indicates that the processes responsible for canyon formation have been active very recently and may still be active in some places. The possibility remains, however, that other submarine canyons, or lower reaches of these canyons, were formed in pre-late Pleistocene time.

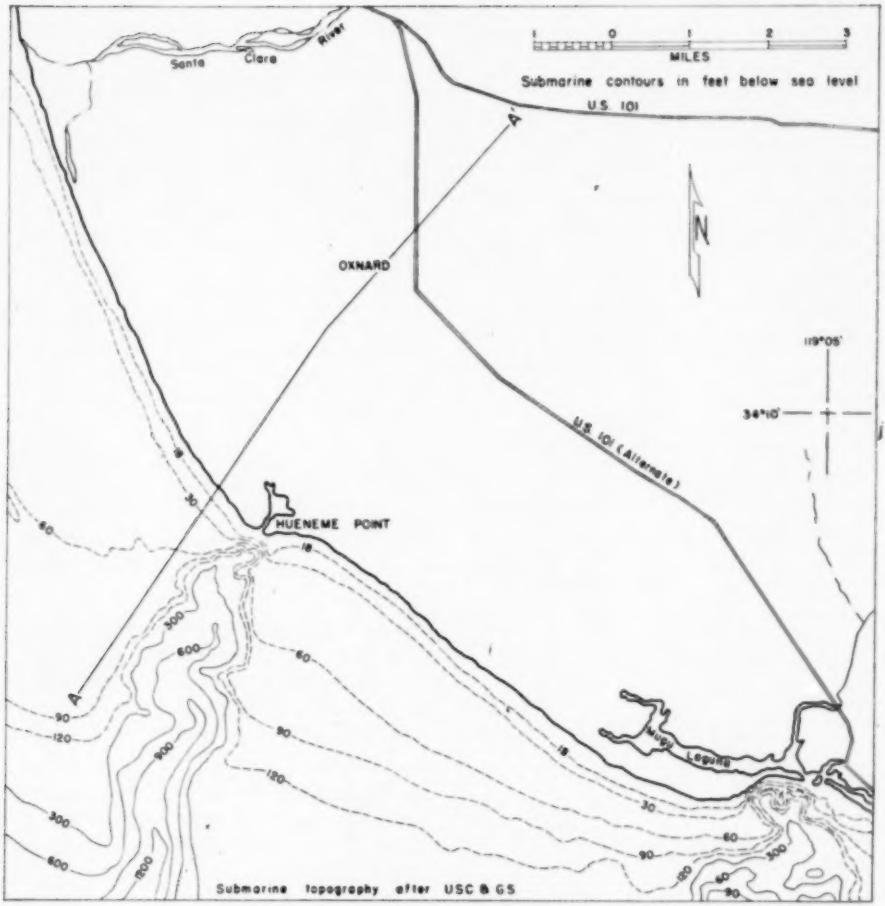
GEOLOGY NEAR HUENEME AND MUGU SUBMARINE CANYONS

Davis (1934, p. 299) called attention to the fact that the Hueneme and Mugu submarine canyons notch the seaward edge of the Oxnard Plain, which appeared to have "long been the site of subsidence." Recently Poland, Garrett, and Mann (1948), who have studied the geology of the plain, state (p. 10):

> The thickness of the alluvial blanket has not been determined, except in a few wells, but in general is believed not to exceed 500 feet. Near the present coast line, the alluvium is interbedded with sandy beach deposits and clayey lagoonal deposits, such as are developing there today. The upper part of the alluvial deposits is of Recent age; the lower part is probably of late Pleistocene age.

This statement suggests that the upper 500 feet or so of the alluvium consists of conformable Upper Pleistocene and Recent beds. Within these strata Poland *et al.* traced an aquifer, which serves to define the structure, from the Santa Clara River southwestward to Port Hueneme (sec. *A–A′*, fig. 5, simplified from Poland *et al.*, 1948, sec. *B–B′*, pl. 3). It appears reasonable to project these beds seaward to the Hueneme Submarine Canyon and conclude that the head of the canyon cuts Upper Pleistocene and probable Recent beds.

It appears likely that Mugu Subma-



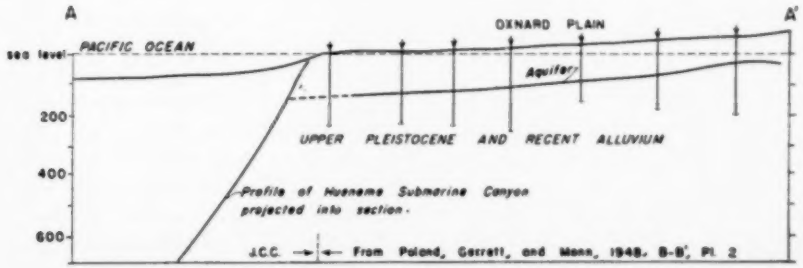


FIG. 5.—Geology in vicinity of Hueneme and Mugu submarine canyons

rine Canyon also truncates the seaward end of Upper Pleistocene sediments, although the data are not so clear. No doubt both canyons, and perhaps the three intervening smaller canyons, cut Lower Pleistocene strata (San Pedro and Santa Barbara formations) which underlie the Upper Pleistocene beds.

GEOLOGY AT REDONDO SUBMARINE CANYON

Recent geological investigations by Poland, Garret, and Sinnott (1948) in the Torrance–Santa Monica area southwest of Los Angeles have cast light upon the origin of Redondo Submarine Canyon. This information, summarized below and in figure 6, suggests that the head of the canyon is cut into Upper Pleistocene and Recent deposits and that *no* land river has flowed to the *upper part* of the canyon during the time of its erosion.

Rocks at Redondo Beach, according to Poland *et al.* (1948), consist of Pliocene and Quaternary clay, silt, sand, and gravel of continental and marine origin. These units, subdivided into several formations, as shown on figure 6, were laid down in a depression north of the Palos Verdes Hills and southwest of the Newport-Inglewood uplift, a zone of faulting and folding intermittently active since the Miocene (Poland *et al.*, 1948, p. 139). After deposition and folding of Upper Pleistocene sediments the coastal plain emerged and channels were cut. In regard to these channels, Poland *et al.* state (1948, p. 50):

> . . . two tongues of fluvial deposits [see "50-foot gravel," *A–A'*, pl. 5] . . . extend from the central lowland (Downey Plain) to the coast. These tongues occupy stream-cut erosional gaps which are known as Ballona Gap and Dominguez Gap. . . . The trench [Ballona Gap] was cut into the late Pleistocene marine (Palos Verdes) surface by an ancestral westward-flowing Los Angeles River and is floored by Recent alluvial deposits to a depth of 50 feet near the coast and to about 80 feet northeast of the Baldwin Hills, about 9 miles upstream.
>
> It is believed that the present Ballona Gap represents an inland segment of the trenching—that is, that the incised stream was graded to a base level substantially more than 50 feet below present sea level and possibly as much as 2 to 3 miles seaward from the present coast.
>
> Dominguez Gap was eroded to a depth at least 150 feet below present sea level at the coast.
>
> The recent epoch of aggradation began with the deposition of gravel and coarse sand to a depth of 50 to 70 feet. Subsequently, deposits of silt and fine sand about 75 feet thick were laid down on top of the permeable basal tongue. Thus the trench was backfilled to a thickness of about 150 feet with deposits of Recent age.

This information indicates that, for a time in the very late Pleistocene, the sea-level in the Redondo region stood relatively somewhere between 75 and 240 feet below present level. The two rivers known to have reached the sea at that time, occupying Ballona and Dominguez gaps, have left a distinct record which fills the time interval between the deposition of the Upper Pleistocene beds and the Recent. Despite numerous wells in the vicinity, no similar channel with a tongue of alluvium has been recognized beneath the plain east of the head of Redondo Canyon. Hence it seems clear that no major stream debouched into Redondo Canyon during or since the time of low sea-level stand.

The head of the canyon is probably no older than late Pleistocene because beds of this age (perhaps even Recent dune-sand and coastal deposits) have been folded below sea-level—below the 25-foot depth where Redondo Canyon can be recognized—and therefore erosion of the canyon head since the late Pleistocene is implied (*A–A'* and *C–C'*, fig. 6). If it is true that submarine canyons, including Redondo, are eroded by submarine processes, as is discussed below,

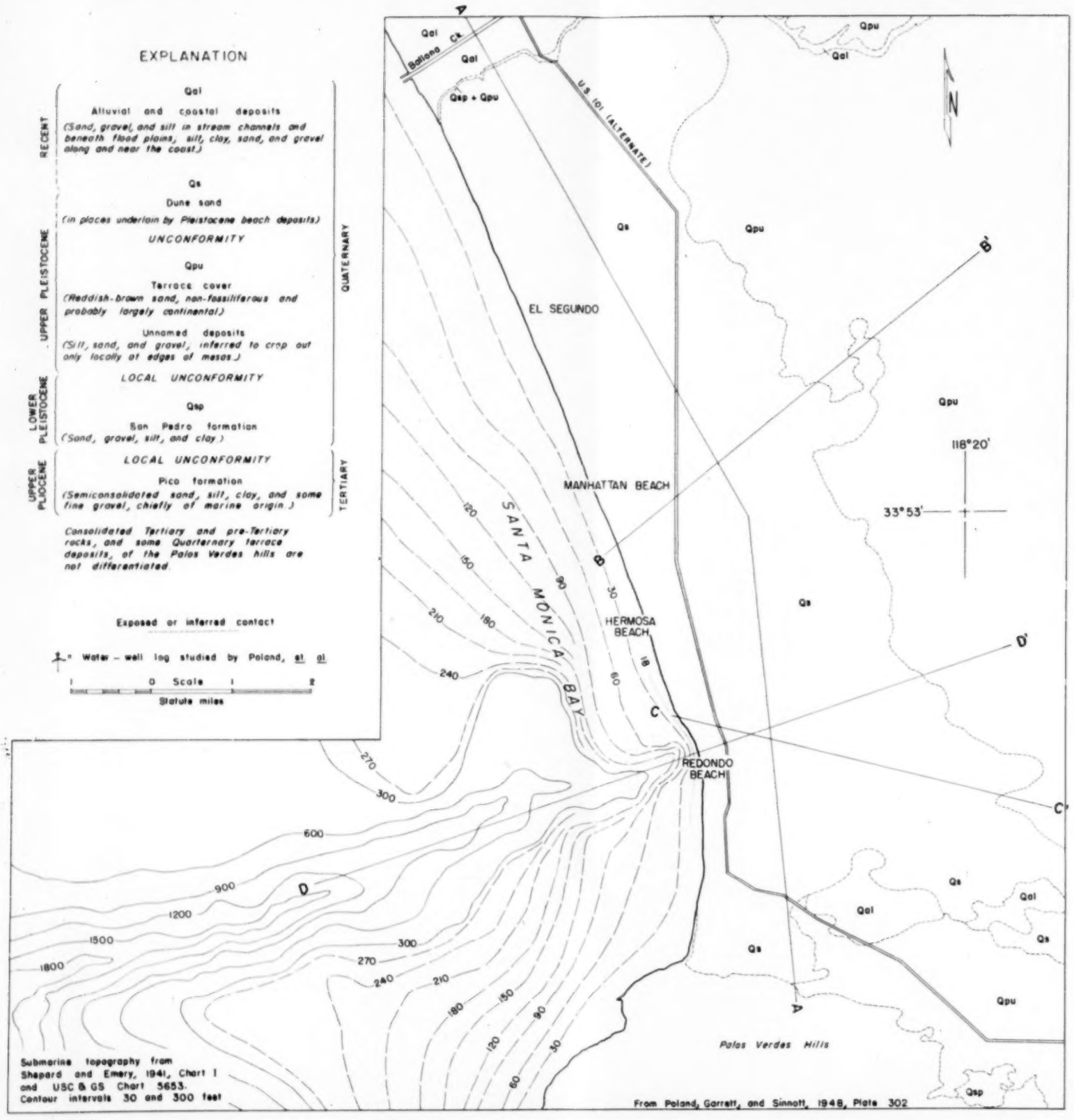
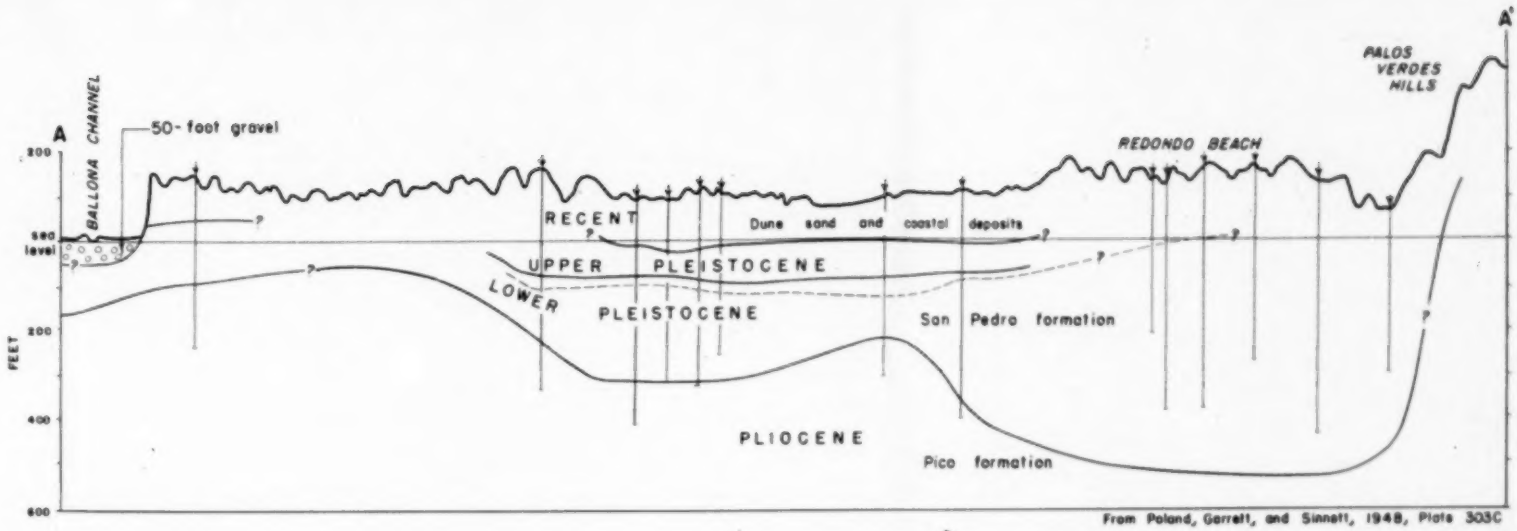
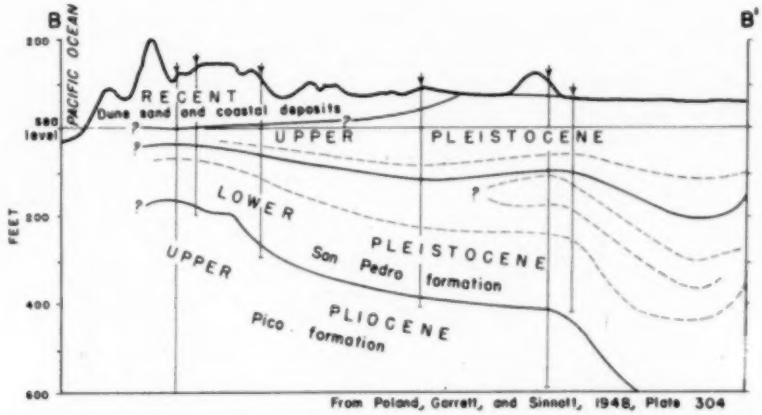
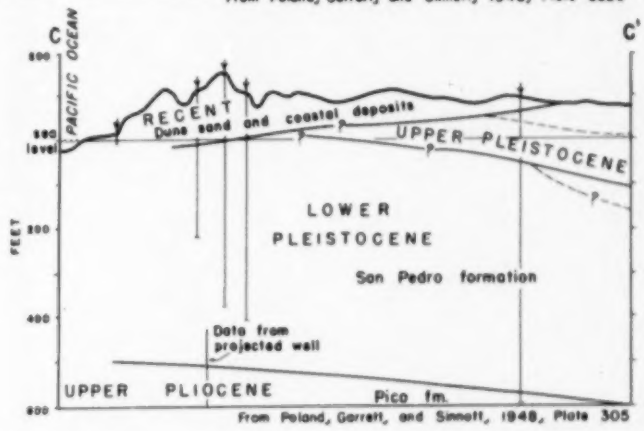
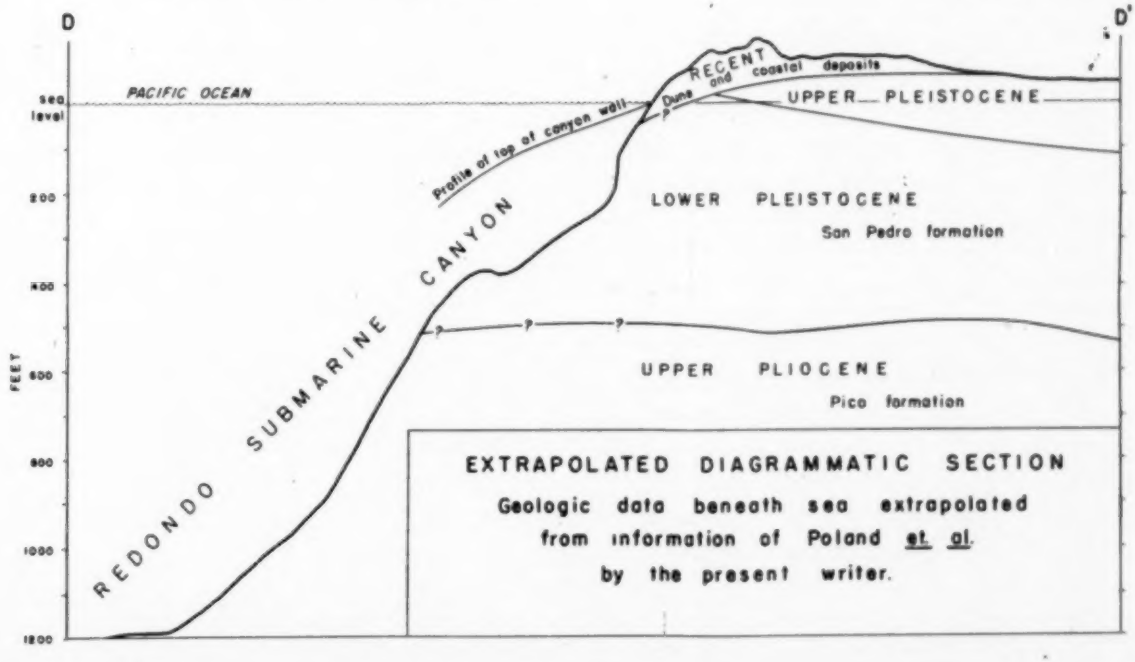


FIG. 6.—Geology at head of Redondo Submarine Ca









ine Canyon, from Poland, Garrett, and Sinnott (1948)

it follows that the head of Redondo Canyon has been eroded since the rise of sea-level following the low stand in the late Pleistocene because the canyon now heads above the channel-bottoms graded to the low level. This implies that the head was eroded in the recent past and may be eroding now.

A similar argument applies to Newport Submarine Canyon. It heads in water less than 50 feet deep (Shepard and Emery, 1941, pl. 13) and extends at least 100 feet above the base of the near-by Talbert water zone. Poland *et al.* (1945, pp. 70, 101, pl. 709, *E–E'*) find this water zone to extend down to about 150 feet below sea-level about 2 miles northwest of the canyon head. Because gravels of the Talbert zone were laid down with rising base level following the low stand of sea in the late Pleistocene, it seems clear that the canyon head has been eroded since that time.

EUSTATIC CHANGES OF SEA-LEVEL

Subsurface information from the Santa Barbara coast, similar to that described by Poland *et al.* (1948) for the Torrance-Redondo area, led Upson (1949) to publish an outstanding contribution on the coastal geomorphology of California. After a detailed study of water-well data, stream courses, and marine terraces, and with attention to areas elsewhere, he concluded (p. 110):

> Along most of the California portion of the Pacific Coast, at least, and possibly also the Oregon portion, streams have apparently carved older valleys below a low marine terrace. These valleys are apparently graded to a relative position of sea level that ranges from 120 to 300 feet below present level. It appears that the larger figure represents the maximum decline of sea level below the present level. These figures, especially the maximum of 300 feet, are of the same order as figures assembled by Antevs (1928, p. 82) and more recently by Flint (1947, pp. 446–447) for various parts of the world. Stearns (1935, p. 1934) has presented similar evidence for a 300–360-foot stand of the sea below present level on Oahu and other islands of the Hawaiian group.

Many of the rivers flowing into the ocean along the part of the coast now under consideration, such as the Santa Maria, Santa Ynez, Ballona, Los Angeles, San Gabriel, Santa Ana, San Luis Rey, and Tia Juana, are deeply alluviated for several miles inland. The upper surface of this alluvium is now graded to present sea-level, but the profile along the base of the alluvium is graded to a point some miles offshore and below sea-level. Upson (1949, p. 111) therefore considers, on what appears to be sound evidence, that this lower level represents the stand of sea-level during the last or Wisconsin glacial stage.

This sequence of events is important to our understanding of submarine canyons. In the first place, despite a wealth of data, no indications of a Pleistocene low stand below 300 feet have been discovered. Second, if sea-level were at one time at a level approximately 6,000 feet lower than today, as suggested by Shepard (1948*b*, p. 248), geomorphic features similar, but on a much larger scale, would be expected. In other words, with exposure of the continental margins, the rivers would have cut their courses deeply, with the lower reaches graded to the low stand of sea-level. With restoration of sea-level near where it is today, these rivers would have filled in their canyons with alluvium to form an estuary. Inasmuch as the trench and tongue of alluvium extend so many miles inland for the relatively small 300-foot change of level described by Upson (1949), evidence of similar trenching or alluviation (but much deeper and more extensive) would also be expected for the rise of 6,000 feet.

None has been detected. In fact, none of the submarine canyons, as noted by Smith (1902) and Woodford (1951), terminates in an estuary.

Other implications of Shepard's "glacial-control and marginal-warping hypothesis" (1948*b*, p. 245) appear at variance with my interpretation of geomorphic events along the California coast. With a marked lowering of sea-level, rivers develop nick points near their mouths, which then move upstream as erosion continues. The position that this nick occupies at any one time is determined by the amount of erosion since rejuvenation, and not by the amount of rejuvenation. The amount of erosion, of course, depends on the rate of erosion and time lapse. Although it is conceivable that the amount of rejuvenation could be constant along the whole California coast, the rate of erosion is a function of stream discharge, gradient, rock resistance, etc.—factors which vary widely. Hence the nicks would have worked headward to different elevations; yet many of the submarine canyons now head almost precisely at sea-level, a remarkable coincidence, so remarkable as to cast doubt on a subaerial origin for the nicks. The elevation and position of the nicks after uplift, it is stressed, are determined by the amount of erosion and are independent of the subsequent rise in sea-level. In addition, as pointed out by Woodford (1951), the nicks have the wrong shape if this coincidence is admitted for the sake of argument. Subaerial erosion processes develop nicks which are convex upward, or rounded, instead of the characteristic shapes shown in figures 2, 3, and 4.

Inasmuch as the long submarine canyons north of Point Arguello extend down to depths of the order of 10,000 feet, relative uplift (coupled, some will argue, with marginal warping) must have been of about the same magnitude if these canyons are subaerial in origin. In addition to those features mentioned above, other effects of this great lowering of sea-level would be expected. For example, Professor Cordell Durrell has asked why the Santa Lucia Mountains have not been glaciated, as they would then have towered about 14,000 feet above sea-level. In addition, Woodford (1951) would expect great gorges between the basins off southern California and the deep ocean beyond, because, with such relative uplift, the basins would have become lakes draining over sills and through gorges to base level far below. Inferred changes in other parts of the world brought about by a marked lowering of sea-level have been discussed by several writers, especially Kuenen (1947).

TABLE 2

If Sea-Level Lowered		Approximate Per Cent of Present Ocean Volume Lost to Icecaps	Approximate Per Cent Increase in Salinity, Relative to Present Salinity, in Remaining Ocean
Meters	Feet		
200	657	5	5
1,000	3,280	24	31
2,000	6,562	46	86
3,000	9,842	66	198

Edward L. Winterer (personal communication) has estimated that the salinity of the ocean would be markedly increased by tying up so much water in Pleistocene icecaps; yet paleontologists and biologists report no record of toxic effect upon sea plants and animals. Using data on areas enclosed between successive isobathic contours given by Littlehales (1932, p. 15), Winterer computed roughly

the volume of water in the present ocean above the 200-, 1,000-, 2,000-, and 3,000-meter depths, respectively. If all salts remained in the ocean, a drop of sea-level of 200 meters (657 feet) would represent a loss in volume to the ocean of about 5 per cent, and the salinity in the remaining ocean would have been increased by more than 5 per cent. For other depths of lowering see table 2.

CONCLUSIONS FROM GEOLOGY AND GEOMORPHOLOGY OF SUBMARINE CANYONS

The purpose of the discussion up to this point has been to show that there are significant differences between submarine and land canyons. It is believed that these differences, together with inferences from the geology at the heads of some canyons and supplemented by the difficulties concomitant with great relative uplift, are so important that a subaerial origin for the submarine canyons is most unlikely. We are therefore forced to turn to *submarine processes* to explain their formation. Topics enlarged upon above, which bear upon this conclusion, are briefly reviewed here:

1. Submarine canyons differ morphologically from land canyons in the following ways:
   a) Their longitudinal profiles are much more irregular. Submarine canyons are not nearly so well graded as are land rivers; many nicks occur at different levels along their courses; and no simple geometric curve, such as the logarithmic curve for land streams (Woodford, 1951) can be applied to them.
   b) Their long profiles are much steeper than the lower reaches of adjacent land canyons and as steep as very steep land canyons.
   c) Tributaries of submarine canyons are usually grouped about the canyon head. This contrasts with the long lower reaches relatively free from them.
   d) Submarine canyons constitute a very small proportion of the submarine slope when compared with the proportion of topography on land occupied by canyons. This suggests that the processes responsible for submarine canyons are not so universal or so efficient as the subaerial processes which cut land canyons.
2. The formation of the following geomorphic features by subaerial processes, involving the relative lowering of sea-level of about 10,000 feet, is difficult to understand.
   a) Many submarine canyons head at or very near the present shore line. Along the California coast, Pleistocene marine terraces now above sea-level are considered to be tilted and warped near Ventura (Putnam, 1942, p. 739); uplifted but not tilted or warped near Santa Barbara (Upson, 1949, p. 108); and warped, tilted, and depressed below sea-level near Redondo Beach (Woodring *et al.*, 1946, p. 109; Poland *et al.*, 1948, pl. 302). For nine canyons to head at present sea-level along such an unstable coast requires a very unlikely coincidence of independent events if a subaerial origin is advocated for them.
   b) For those submarine canyons which head near the mouths of rivers, a sharp nick in the combined long profiles stands out strikingly (figs. 2 and 4). Diverse elevations of these nicks could be expected only with subaerial erosion,

yet the nicks now occur at or very near present sea-level.

c) Submarine canyons appear to be graded (admittedly poorly) to many different levels (figs. 2, 3, and 4) down to more than 10,000 feet subsea. The level for each canyon seems to correspond to the local sharp break in slope at the base of the steep basin wall or continental slope. If the canyons were graded to low stands of the sea, greater uniformity in the depths to their mouths and nicks would be expected.

d) Many submarine canyons show no relation to rivers on land. In addition, at Redondo Canyon, where the head of the canyon was cut during the late Pleistocene and Recent, no river has been discovered which could have eroded the canyon head during the time required.

e) With an effective lowering of sea-level of about 10,000 feet, other geomorphic features would be expected which have not been discovered. Among these are gorges between basins off southern California, evidences of glaciation in the Santa Lucia Range, and indirect evidences of increased salinity in the remaining ocean.

3. Recent studies by Upson (1949) and others show that several large California coastal rivers, now alluviated and graded to sea-level, were, during the late Pleistocene, graded to a horizon now 200–400 feet below sea-level. None of the submarine canyons heads into an estuary which shows similar geomorphic features, but on a much larger scale.

a) Redondo and Newport submarine canyons head in water less than 50 feet deep, *above* the base of the near-by alluviated channels eroded during the late Pleistocene. If submarine processes are responsible for cutting the canyon heads, these relations show that *erosion has taken place since the late Pleistocene.*

4. To the writer's knowledge, no structural evidence to support the idea of the warping of continental margins has yet been discovered (Shepard, 1948*b*, p. 245; Emery, 1950, p. 103).

These data lead the writer to conclude, as Smith (1902) and Woodford (1951) have before him, that submarine canyons along the California coast have been eroded *by submarine processes* and, further, that the heads of some of the canyons have been eroded *by submarine processes in operation today*. In the remaining parts of this paper attention will be focused on sediment transportation in the ocean and its probable bearing on canyon origin.

SEDIMENT TRANSPORTATION ALONG THE CALIFORNIA COAST

Most of the sediment available for transportation in near-shore waters off California comes from rivers and is not worn from sea cliffs by wave-action (Shepard, 1948*b*, p. 96; Handin, 1951, p. 25). After the sediment is brought to the ocean by streams, it is moved about by oceanic processes. Much is carried parallel to shore by longshore currents, which gain their drive from breaking waves. Coarse material, such as sand, finds temporary rest upon beaches; but it, too, drifts in the direction of the prevailing longshore currents. This drift is apparently superimposed upon a seasonal movement normal to the beach, which carries material into deeper water during the winter, when wave-action is more in-

tense, and then into the beaches during the summer (Shepard and La Fond, 1940; Shepard, 1948*b*, chap. 5). In view of the turbulence in the surf zone, fine sand, silt, and clay are probably moved almost continuously and do not come to rest on the beaches except in sheltered areas where wave energy is low. The fine material, together with the sand drifting along the beach, continues to move until the transporting power of the longshore current is markedly reduced or until the current meets another coming along the coast from the other direction. In either case the capacity of the longshore current may be exceeded, bringing about deposition of the debris or seaward movement. Rip currents, for example, which often result where two longshore currents converge (Shepard, Emery, and La Fond, 1941; Shepard, 1948*b*, p. 43), succeed in moving water—and, no doubt, some sediment—from the surf zone toward deeper water. In the absence of an efficient current to move the debris farther along the coast, what happens to this sediment? Is it possible that the sediment accumulates in these areas and, if the slope is steep enough, eventually slides seaward? Are submarine canyons found at such localities? Before considering these questions, a further discussion of shore processes is desirable.

As waves break, their energy is dissipated in several ways. Most is lost in turbulence within the surf zone and in forcing the uprush onto the beach. An indeterminate amount, probably small, is converted into heat. But where there is an angle between the breakers and the shore line, a component of momentum exists parallel to the coast. Some of the energy from the breaking waves will thus be available to propel a current along the shore. This current, which reaches its maximum velocity at the point of breaking, is the *longshore current*.

Factors involved in the generation of longshore currents have recently been discussed by Putnam, Munk, and Traylor (1949). The *direction* of the current is determined by the angle between the wave crests at the breaking point and the beach contours; the current is propelled out of the open angle. The *strength* of the current is determined primarily by the size of this angle and the height of the breakers, although the period of the waves, the slope of the beach, and the character of the bottom are also important. For the California coast it is possible to predict the direction of the longshore current if the direction of wave approach to the shore line can be determined at each point.

Data on the direction of wave approach to the California coast have been compiled recently by a group at the Scripps Institution of Oceanography (1947). Techniques developed during the war (U.S. Hydrographic Office, 1943) for forecasting swell height, period, and direction of travel were applied to three years (1936–1938) of daily weather maps covering the northern Pacific Ocean, where most of the waves arriving at California originate. Although this method disregards such factors as the inshore effects on the swell, following and opposing winds, and swell originating in the Southern Hemisphere and is based on weather analyses over an ocean where observations are scarce, it nevertheless provides very useful information. Wave roses summarizing these data appear in figure 7. There can be little doubt that the information is accurate enough to indicate the average direction of approach of the waves to the coast. Although wind waves, raised over fetches just offshore are from time to time large enough to de-

termine the direction of the longshore current, their omission from consideration here probably does not seriously affect our conclusions.

Figure 7 has been drawn with this wave information as a base, allowing qualitatively for refraction inshore (U.S. Hydrographic Office, 1944; Munk and Traylor, 1947). It shows wave crests as they approach the shore from three principal directions selected from the wave roses. For each of the three directions of wave approach the *direction* of the longshore current, and therefore the longshore drift of sediment, is shown by means of small arrows in red. For the northwest and west-northwest direction of wave approach the prevailing direction of drift is from the northwest toward the southeast, except in Monterey Bay and along the upcoast sides of prominent headlands, such as Buchon Point, Rocky Point, etc. (fig. 1). Westerly waves, on the other hand, cause local reversals from Monterey Bay to Point Arguello and in Santa Monica Bay.

Although only three directions of wave approach are shown in figure 7, waves arriving *between* the directions selected need attention also. South of Point Arguello, for example, bands free from waves are shown behind coastal headlands and islands. These bands have been drawn on the assumption that the waves came from one direction only, whereas, in fact, they come from directions scattered about those selected. The effect of these other exposures on particular beaches can be appreciated, however, by noting the directions sheltered by the islands and headlands and by comparing the orientation of the shore line with those directions whence waves can arrive between islands. Except where the nearshore topography of the sea floor is complicated, the probable direction of the longshore current for any direction of wave approach can be determined. Waves traveling toward the beach along a direction to the left of a line normal to the beach (looking toward the beach) impel the current to the right; those traveling along a direction to the right drive the current to the left.

South of Point Arguello the shape of the coast and the islands largely protect the beaches from the onslaught of the northwesterly and west-northwesterly waves. Because of these factors, waves approaching from the west and from other directions become statistically more important. The trend of the coast, however, precludes a general northward drift of shore sediments unless the waves approach from the south, an unimportant direction of wave approach except occasionally in the summertime during the arrival of swell from storms in the Southern Hemisphere. Waves approaching from the west through southwest cause local reversals.

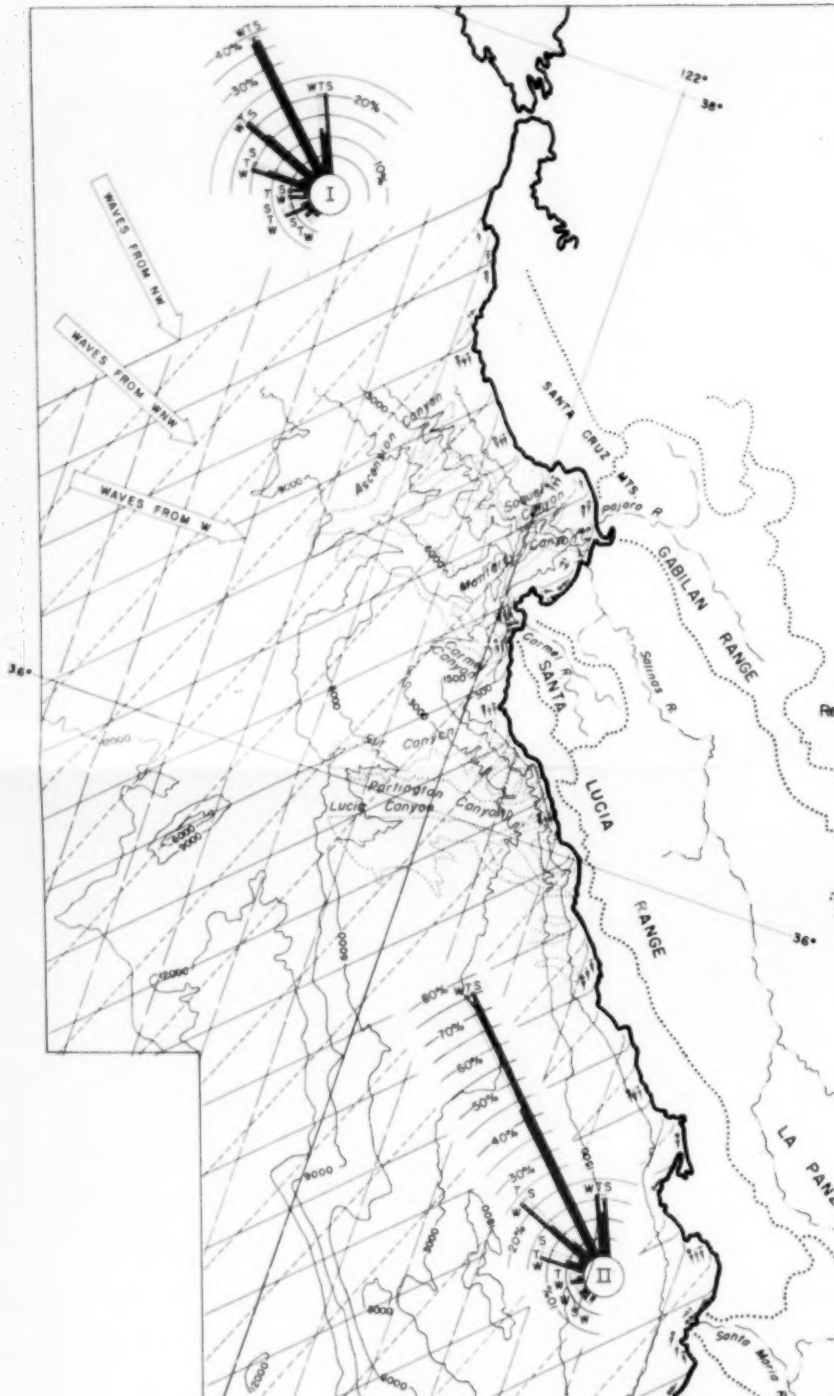
Study of figure 7 reveals the following:

1. Most of the swell arriving at the coast of southern California approaches from directions north of west-southwest.

2. With such directions of wave approach, the prevailing longshore drift is down-coast, or from the northwest toward the southeast.

3. For the three directions northwest, west-northwest, and west, beaches are shown where current reversal occurs. Any change in the trend of the shore line affects the size of the angle between crests of breaking waves and the shore line and also the wave energy arriving at the shore due to wave refraction. Changes of these two factors result in changes of the capacity of the longshore currents to carry sediment.

Independent evidence on the drift of sediment southward along the coast



EXPLANATION

Land drainage from USC & GS, 1946, Sectional Aeronautical Charts, RS-1, R-2, & Q-2

= Drainage divide

Submarine topography from Shepard & Emery, 1941, Charts I, II, III.

Contours shown : Subsea, 300, 1500, 3000, 6000, 9000, 12000 Feet.

Outline of submarine canyon

Wave crests for three directions of wave approach (NW, WNW, and W) shown in red with directions of longshore currents resulting from these waves

→ Longshore current direction when waves from NW

→ Longshore current direction when waves from WNW

→ Longshore current direction when waves from W

Refraction of waves near shore, and sheltering effects of islands and headlands shown diagrammatically.

Wave directions, based on University of California, Scripps Institution of Oceanography, Wave Report No. 68, 1948, are shown for these three localities:

I	37.5° N. Latitude	123° W. Longitude
II	35.0° N. Latitude	121° W. Longitude
III	33.0° N. Latitude	120° W. Longitude

Wave roses give percentage frequency of occurrence of waves arriving within 22.5° sectors centered on directions shown, by seasons as follows:

W = Winter months (Dec., Jan., Feb., March for 1936, 1937, and 1938)

S = Summer months (June, July, Aug., Sept. for 1936, 1937, and 1938)

T = Transition months (Apr., May, Oct., Nov. for 1936, 1937, and 1938)

10 0 10 20 30 40

Statute Miles

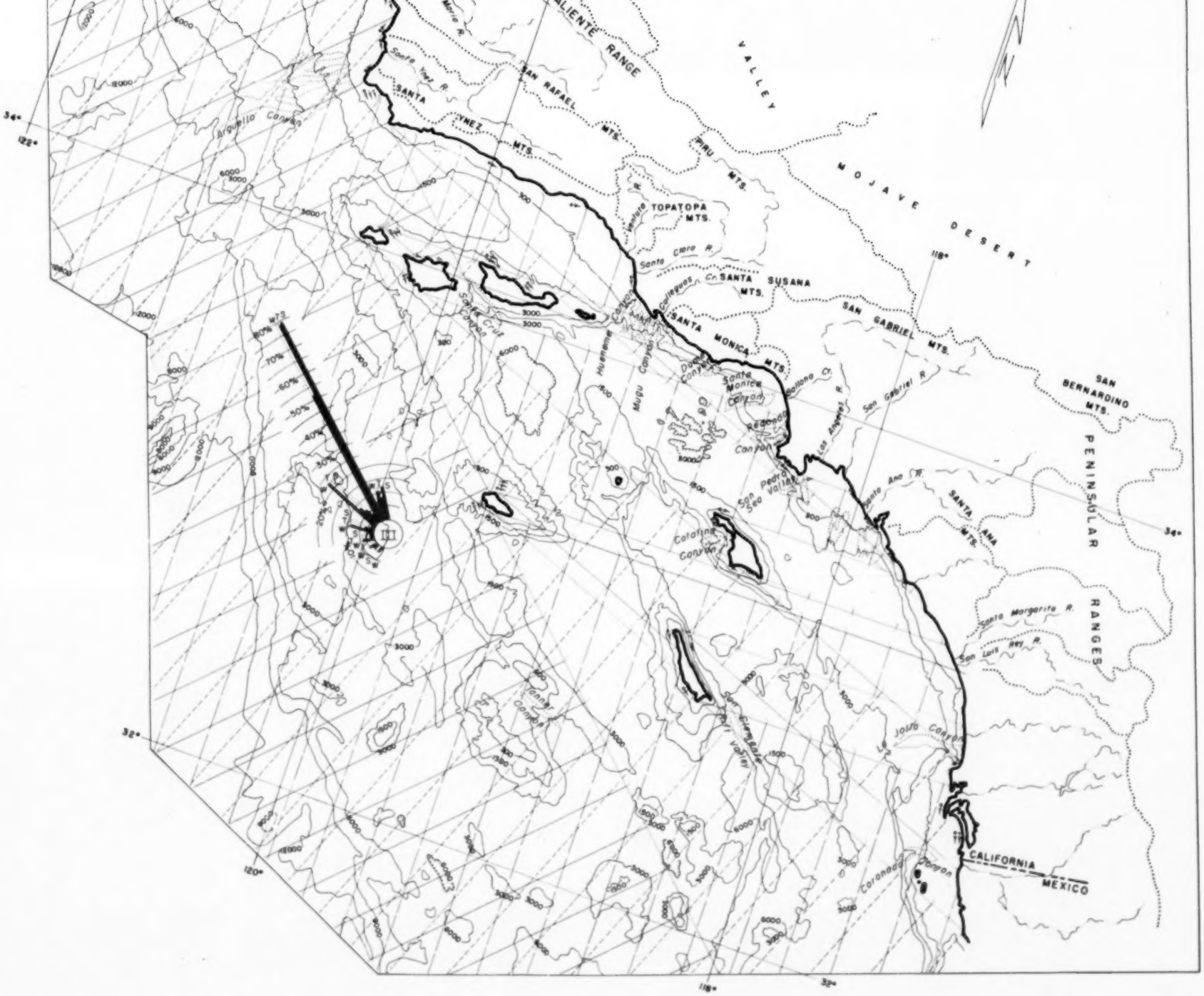


FIG. 7.—Wave-approach diagram, with submarine topography and coastal drainage areas of central and southern California

comes from other sources. Observation of groins at several places shows accretion of sand on the northern (or western) side and erosion on the down-coast side. Excellent photographs of such groins and breakwaters may be found in Shepard (1948*b*, pp. 323–325). In addition, the eastward movement of sand in the vicinity of Santa Barbara is discussed in a paper published by the Beach Erosion Board (1938).

Handin (1951) discusses at some length the source and disposition of sediment reaching the coast along the Oxnard Plain. Estimates of the amounts of sediment contributed by the Santa Clara River, brought in from the coast to the northwest, blown inland from the beaches by deflation, moved southeastward along the shore by wave-action, or used to prograde the beaches, still leave a significant proportion unaccounted for, which must be moved offshore to deep water in the vicinity of Hueneme Submarine Canyon. Handin's data (1951, p. 36) do not indicate whether this sediment moves out from shore by some as yet unknown process or is carried to the head of Hueneme Canyon. Gradual changes in sediment character and composition along beaches here and elsewhere suggest, however, that some material is carried seaward from the beaches, whether or not a submarine canyon is near by. Hence an unknown proportion of the sediment arrives in the vicinity of the canyon heads.

In regard to Santa Monica Bay, Handin states (1951, p. 55):

> Beach sands entering Santa Monica Bay from the west of Point Dume and from tributary streams cannot escape from the confines of the bay by littoral drifting, so that they must ultimately accumulate somewhere within the bay, or be carried off shore beyond the limits of the continental shelf, or be blown inland. In early historic time when the up coast source of beach sand was augmented by the Los Angeles River, much sand accumulated in the dunes along the southern coast of the bay. These dunes are not now active. Under normal conditions the beaches did not prograde during historic time. At Redondo it seems clear that finer sands at least are moved off shore by rip currents to be deposited in Redondo submarine canyon. Since both the competence and capacity of longshore currents decreases with distance down coast from Santa Monica, material must be deposited, and since the beaches were normally stable, the material must be deposited off shore or in the coastal dune area. Bottom deposits in southern Santa Monica Bay consist of fine sand for a distance of several miles off shore.

At Redondo, therefore, we note that a submarine canyon heads in near shore at a place where sediment is accumulating, owing to the reduction of the capacity of longshore currents and other attendant processes. In fact, at Redondo, longshore currents converge to form rip currents or to block the further down-coast movement of beach sediment. This coincidence between diminished capacity of longshore currents and the presence of a submarine canyon is believed to be related and causal.

A glance at figure 7 or at Shepard and Emery's chart I (1942) brings out another relation which may bear upon the location of submarine canyons. Along the coast eastward from Santa Barbara no canyons occur until Point Hueneme is reached. Over this distance the gradient of the offshore submarine slope is very gentle, but at Point Hueneme a steeper escarpment is relatively near the shore line. A steep slope as a further requirement for canyon development is accordingly suggested.

CONDITIONS FOR THE FORMATION OF CALIFORNIA SUBMARINE CANYONS

It is appropriate here to summarize what appear to be the conditions at-

tending the development of submarine canyons along the California coast. These conditions are three:

1. A SOURCE OF SEDIMENT

Because the bulk of sediment near shore comes from rivers, it is to be expected that submarine canyons will be situated near mouths of large rivers, if the other two conditions are fulfilled. But near-by rivers are not necessary if a flow of sediment along shore is maintained by strong wave-action. Sediment brought down the Santa Clara River, for example, is moved by longshore currents until current capacity is reduced by the shape of the shore line at Point Hueneme. Although some is moved around the point, a substantial amount may accumulate at the head of Hueneme Submarine Canyon and eventually move seaward down the canyon to deep water.

Drainage areas of land river systems are outlined in figure 7, in order to permit a rough evaluation of the amount of sediment brought to the ocean. Note that the large submarine canyons heading in the vicinity of Monterey Bay are located where they may receive large amounts of sediment from the Salinas River, the largest stream reaching the coast under consideration here. Note, too, that the long reach between Lucia and Arguello canyons without submarine canyons receives relatively little sediment.

2. A NEAR-SHORE STEEP SUBMARINE SLOPE

Although we do not at present understand the actual process of canyon-cutting, steep offshore slopes heading relatively near the shore line appear to be found wherever submarine canyons occur. If sediment scours the canyons as it moves seaward under gravity, it is understandable how the scouring would be more efficient if the submarine slope were relatively steep. This requirement also appears to dovetail with the observation, enlarged upon above, that gradients of submarine canyons are steeper than those of land rivers.

In connection with submarine canyon erosion processes, Daly (1942, p. 144) and others have discussed suspension or density currents observed in Lake Mead, impounded behind Hoover Dam on the Colorado River. Sediment-laden water, which enters the lake about 120 miles above the dam and flows along the floor of the reservoir as a distinct bottom current, can be recognized downstream at the dam some days later. It is clear that the suspension current moves over this distance without losing its identity. In Lake Mead, it is pointed out here, erosion of the lake floor or old river bed could hardly be expected. According to profiles measured by the U.S. Geological Survey (1924, Sheets S and T), the average gradient of the Colorado River over this 120-mile sector is only 4.7 feet per mile, or about one-thirtieth as steep as the average gradient of La Jolla Submarine Canyon.

3. REDUCTION IN LONGSHORE CURRENT CAPACITY

With reduction of the ability of longshore currents and associated processes to move sediment near the shore, sediment will accumulate. If the slope is steep enough or if a submarine canyon is already developed, sediment may then move seaward and, in so doing, erode.

Two of these three conditions—steep topography and adequate sediment—appear to be of major significance, as nearly all submarine canyons occur where these conditions are fulfilled. A few canyons, however, such as Partington, are found where longshore current

slackening is not marked, suggesting that the third condition is least important. In addition, at least two other factors need emphasis: first, canyon courses appear to be influenced by rock resistance and structure as on land, and, second, submarine processes probably tend to perpetuate the canyons, once they come into existence. This no doubt means that the past history of a canyon, often complicated, affects its appearance today.

DISCUSSION OF INDIVIDUAL SUBMARINE CANYONS

Each of the submarine canyons bordering southern and central California is discussed here with respect to the three conditions of origin described above. Figure 7 and Shepard and Emery's charts I, II, and III will be helpful in reading. The writer realizes that many statements below are oversimplified and are perhaps unwarranted in view of our present knowledge; they are nevertheless advanced in order to focus attention on the good points as well as the shortcomings of the theory of canyon origin presented here.

SUBMARINE CANYONS IN AND NEAR MONTEREY BAY

Several large submarine canyons head in Monterey Bay or notch the continental shelf near by. Sediment probably reaches these canyons from two principal sources: first, from the Salinas, Pajaro, and Carmel rivers and the much smaller coastal streams of the Santa Cruz Mountains (fig. 7) and, second, from along the coast north of Point Año Nuevo (fig. 1). In view of the prevailing southeastward drift of beach sediment northwest of Monterey Bay (fig. 7), any sediment now contributed to the group of canyons between Ascension and Soquel probably comes from up the coast to the northwest. Judging from figure 7, it is likely that a considerable proportion of the sediment now moving along the coast by-passes these canyons to Soquel and Monterey canyons. Because the group between Ascension and Soquel does not head right at the present shore line, it is uncertain to what extent these canyons are now being eroded. Perhaps they were cut chiefly during the late Pleistocene when sea-level stood about 300 feet lower than today and the continental shelf was only a few miles wide. In addition, the canyons may have originated in pre-late Pleistocene time and may have undergone modification with changing conditions since then.

Two submarine canyons head right at the shore line in the Monterey Bay district, Monterey and Carmel, and are therefore assumed here to be actively eroding headward. For them the three conditions seem well established: an ample supply of sediment is dumped into the ocean by the Salinas, Pajaro, and Carmel rivers, supplemented by drift down the coast from the northwest. In fact, more sediment is certainly contributed to these canyon systems than to any others along the California coast. Because the continental shelf is relatively narrow at Monterey Bay (narrower than anywhere up-coast within at least 100 miles), and the deep oceanic floor (10,000–12,000 feet subsea) extends so near shore, a remarkably steep submarine slope is possible. Moreover, the shape of the bay and the frequent opposition of longshore currents in the bay (fig. 7) cause the accumulation of sediment. Note the relations between the shape of the shore line, the point of current convergence with westerly waves, and the position of the head of Monterey Canyon.

A large part of the sediment contributed to Carmel Submarine Canyon probably comes around Point Pinos from the northeast, augmented by debris from the Carmel River. Downcoast movement of the sediment is apparently blocked largely by the headland bordering the bay on the south. Bedrock structural control of canyon erosion is suggested by the shape of Carmel Canyon, which bends northward away from shore to join Monterey Canyon as a "barbed tributary."

SUR, PARTINGTON, AND LUCIA SUBMARINE CANYONS

Of the three submarine canyon systems—Sur, Partington, and Lucia—only Partington heads at the shore line and is therefore considered to be actively eroding at the present time. Note that it is situated precisely where the continental shelf is narrowest, although no coastal features likely to reduce longshore current capacity are recognized. Sediment arriving at Partington probably has two sources: one, the streams from the Santa Lucia Range, including the Big Sur, and, two, the beaches up the coast to the northwest, including sediment which escaped Carmel and Sur canyons, as well as offshore dispersal.

Sur and Lucia submarine canyons, because they head some distance from shore, are probably eroding relatively slowly at present. On the other hand, during the Wisconsin stage, erosion may have been quite active. Sur Canyon, it is suggested, is in the lee of Point Sur because of the extension of shoal water for several miles just off the point. South of the point, deep water reaches in near to shore, so steep gradients are possible. An explanation for the position of the head of Lucia Canyon, however, does not come to mind.

ARGUELLO SUBMARINE CANYON

Why are there no significant submarine canyons between Lucia and Arguello? A few comments are here offered toward an eventual explanation. In the first place, proceeding southward from the vicinity of the head of Lucia Canyon, the offshore slope becomes increasingly more gentle and perhaps too gentle for canyon formation. South of the Santa Lucia Bank, however, and near Arguello Submarine Canyon, the inshore slope steepens up. In the second place, relatively little sediment is contributed to this stretch of coast, owing to the small drainage area on the flanks of the Santa Lucia Range. Not until the Santa Maria River is reached does drainage extend inland more than a few miles. Arguello Canyon is therefore situated with both a critical steepness of the submarine slope and a sediment source: the Santa Maria and Santa Ynez rivers, augmented by down-coast drift from the northwest. Arguello Canyon does not, however, extend in close to shore, and data assembled by Upson (1949, p. 112; fig. 3) suggest to me that it was eroded most actively during the Wisconsin low stand of sea-level when the near-shore shelf was much narrower. Perhaps now a large proportion of sediment either rounds Point Arguello and Point Conception or is dispersed to deep water offshore.

HUENEME AND MUGU SUBMARINE CANYONS

It is believed, as mentioned above, that no submarine canyons occur between Santa Barbara and Hueneme because offshore slopes are gentle. In addition, between Point Arguello and the Ventura River, little drainage reaches the sea. Hence Hueneme Canyon is found (1) down-coast from a bountiful supply of sediment; (2) where the submarine topography permits a steep gradient; and (3) where a slight change in shore-line trend reduces longshore current capacity. Mugu Canyon appears to be situated similarly, which implies that appreciable sediment by-passes the heads of Hueneme and intervening canyons. Geomorphic effects of the Wisconsin low stand of sea-level have not yet been recognized in the Oxnard Plain.

DUMÉ SUBMARINE CANYON

Sediment moving eastward toward Point Dume perhaps accumulates near the point because wave action can maintain only weak eastward currents around the headland. In addition, right at the point, the submarine slope is fairly abrupt.

SANTA MONICA AND REDONDO SUBMARINE CANYONS

In Recent times much of the sediment moved along the beaches of Santa Monica Bay has reached the vicinity of the head of Redondo Submarine Canyon, discussed above. During the time of the 300-foot lower sea-level, however, the bay no doubt had a somewhat different shape, and longshore current convergence may have occurred near the head of Santa Monica Canyon.

SUBMARINE CANYONS OF SAN PEDRO BAY

At the present time, only Newport Submarine Canyon extends in to the shore line within San Pedro Bay. It is situated down-coast from several river mouths and where the submarine escarpment swings in close to shore. In addition, longshore sediment transportation probably falters at the slight coastal salient near the canyon head.

During the Wisconsin low stand of sea-level and perhaps during the rise thereafter, the submarine canyon in San Pedro Sea Valley and the two canyons which notch the shelf south of the mouth of the San Gabriel River were probably actively eroded. San Pedro Sea Valley itself, judging from Shepard and Emery's chart I, appears to be partly tectonic in origin.

LA JOLLA, CORONADO, AND CATALINA SUBMARINE CANYONS

Proceeding southeastward along the coast, the next significant submarine canyon, La Jolla Canyon, is found at the first topographic restriction to sediment movement, the bay north of Point La Jolla. At Dana Point, where deep water is relatively near shore and current capacity is reduced east of the point, a series of small canyons is developing. Two other small

ones are present in the vicinity of the mouths of the Santa Margarita and San Luis Rey rivers, where the near-shore shelf is narrow. Of these two, the southern is believed to be more fortunately situated and may receive sediment from these two rivers. In the main, however, most of the sediment moved along the coast must eventually drift to the vicinity of La Jolla Canyon, and it is doubtful if very much gets beyond.

Coronado Submarine Canyon lies between two shoals, with its head some miles west of the present coast, in water about 200 feet deep. It is therefore probably not eroding actively now, and it, too, may have received an ample supply of coastal sediment during the Wisconsin low stand of sea-level. At that time the topography near the canyon head may have been somewhat like that today at La Jolla, with the Coronados Islands culminating a headland on the south.

Catalina Canyon, like Redondo, appears to be localized by topographic influence on longshore currents. San Clemente "Rift Valley," so termed by Shepard and Emery (1941, chart I, p. 21), although located along a probably fault zone, can perhaps be explained by submarine erosion according to the theory set forth here. Sediment carried by wave-action along either coast of the island would arrive at the southern end. Two prominent points at the south probably interfere with current capacity, but the one on the west has a wide shoal area surrounding it. The southeastern point of the island, however, juts into deep water, where steep gradients occur. Sediment accumulating here has perhaps eroded the canyon by moving seaward along the band of weakened rocks in the fault zone.

OFFSHORE SUBMARINE CANYONS

Several small submarine canyons, including Tanner Canyon, are shown on Shepard and Emery's charts. These, it is contended here, have employed local sediments acquired in part from wave-action and may be eroding at the present time. Some banks were no doubt islands when sea-level was 300 feet lower; and such canyons located on their flanks may be akin in origin to those off islands today.

All along the coast small submarine canyons which notch the edge of the continental slope appear on Shepard and Emery's charts. Perhaps these, too, have been eroded by much the same process as those near shore, namely, the downslope movement of sediment under gravity. Many of them, however, are so far from shore that it is difficult to understand how sediments were moved from inshore out to such distances, even after allowing for the decrease in these distances by the seaward retreat of the shore line during the Wisconsin low stand of sea-level.

SUBMARINE CANYON EROSION PROCESSES

To date, little information has come to light which explains satisfactorily *how* submarine canyons are eroded. Although laboratory experiment with careful attention to scale factors may help our understanding of erosion processes, the analytical study of field data acquired chiefly from submarine surveys will no doubt be most useful. Kuenen (1937, 1938) has been the principal student of erosion processes in the laboratory, but as yet his work has not established the efficacy of turbidity currents as cutting agents. Several workers, especially Veatch and Smith (1939) and Stetson (1949), have collected and studied data from submarine surveys. Despite this work and discussion in many papers, including the present one, of the implications of assembled or published information, the question of how the canyons are eroded is still unanswered.

In my view we are forced to look for processes in operation today within the sea in order to explain the California canyons. In addition, it seems clear that the submarine process or processes responsible for eroding most of the California submarine canyons require an unusually steep slope. This implies that gravity is a motivating force. Inasmuch as canyons head where sediment is accumulating and some evidence (Handin, 1951, p. 56) shows that offshore losses of sediment are appreciable and in part take place near canyon heads, the seaward disposal of sediment down canyons is suggested. It therefore seems likely that our process is some kind of sedi-

ment flow, slide, suspension current, density current, turbidity current, or a combination of these. Are there data at hand pointing toward such a process?

Shepard and Emery (1941, pp. 94–103) describe present-day changes at the heads of La Jolla, Scripps, Newport, Carmel, Monterey, and Redondo canyons, each one of which extends to very near the present shore line. The largest change occurred at the head of La Jolla Submarine Canyon (p. 96), where a total deepening of 57 feet was observed. Concerning changes at La Jolla, they state (p. 97):

> The changes at . . . established points show a breaking away from the wall of a large mass of material. The soundings on November 25 [1937] appear to have been taken in the midst of the movement and show that a large mass had been displaced from the wall and a lesser amount was deposited in the canyon bottom. The soundings on December 2 [1937] show that a crack opened up near the base of the slope with a hump outside, as if the material had pulled away from the wall. Soundings on December 6 failed to show any sign of the hump so that it probably slid down the floor of the canyon and out to sea.

Concerning shifts in the head of Scripps Canyon, Shepard and Emery (1941) state (p. 98):

> By far the best established depth change based on soundings took place in Scripps Canyon in the spring of 1938. Previous to this change soundings had been repeated along several ranges, and most of the data suggested shoaling. Reliable soundings on April 29 indicated deepening along three range lines. Large waves on April 30 prevented completion of the resurvey of all the lines until May 5, when a progressive deepening had occurred in two lines.

About Carmel Canyon fill they write (p. 100):

> The head of Carmel Canyon lies within a stone's throw of the beach. In sounding the inner lines one has the impression of being right next to the breaking waves. The steep beach inside with its coarse sand looks as though it would be most unstable and could easily be swept into the canyon head. It was interesting, therefore, to find that between July 1934 and April 1938 there had been a fill of almost 12 feet along the floor of the canyon in the inner section. In August 1938 soundings showed no increased fill, but in August 1939 more fill had taken place. The net fill from 1934 to 1939 amounted to 18 feet in much of the section. During the same time a section 260 feet farther out showed a large fill on the south wall and a fill on the bottom of about 18 feet. An intermediate line also showed a fill between April 1938 and August 1939.
>
> This filling of the Carmel Canyon is so rapid that the head will be largely filled in a few years unless there is a slide.

These observations of distinct changes in six California submarine canyons provide indisputable evidence of the accumulation and occasional disposal of sediment in the heads of the canyons. Shepard and Emery (1941, pp. 96, 119) consider that slides may be a contributory, but not the major, cause of canyons. They further believe that the canyons are kept open by some kind of submarine slumping process or current. I would like to extend these conclusions, however, and suggest that *these are the processes that are actually responsible for eroding the canyons*. Perhaps sediment, sliding or slumping to start with, somehow becomes converted to a suspension, density, or turbidity current. Evidence to prove erosion by such currents will be difficult to acquire, however, although Shepard and Emery (1941, p. 119) point out that "in the case of La Jolla Canyon, [slides] have enlarged a canyon head."

Can we expect to observe the floor of a submarine canyon in the actual process of being eroded? It hardly seems likely. Even though it is demonstrated that vast amounts of sediment move through the canyons, which the writer believes nearly tantamount to proof of erosion by these moving sediments, doubt will

still remain that the moving sediments actually do the eroding. Proof, such as detailed observations of changes in the configuration of hard-rock bottom before and after a time of moving sediment, will be difficult to get, but perhaps will be obtained in the future as our means of making submarine observations are improved.

No doubt the canyons are eroded at infrequent intervals. Davis (1934, p. 300) states in connection with the erosion of submarine canyons:

> The case would be analogous to that of a river, which scours away the sediments of its bed while in flood but lays down similar sediments again as the flood subsides; or to that of shore waves, which in calm weather play about on the beaches, the detritus of which they lift and shift during storms; or to that of water erosion in the desert, which is inoperative for years of no rainfall but which becomes violently active for a few hours after a cloudburst.

It thus seems reasonable that the submarine canyons may be eroded only when the accumulation of sediment in the headward regions becomes unstable and moves seaward. Between these times the floor of the canyon would be covered with debris, perhaps unsorted if the process were one of sliding or slumping, but sorted if suspension or turbidity currents operate. Currents measured in the canyon at such times, like wind measurements above the floor of a dry desert canyon between floods, would have no relation to the torrent of sediment responsible for eroding the canyon.

Although Ritter in 1902 (p. 59) showed that inshore material was carried to deep water down submarine canyons, it is certainly desirable to make further observations of this kind. It would be helpful not only to re-establish down-canyon movement of sediment but to prove whence the material came, and roughly in what amounts. Distinctive heavy minerals, characteristic of certain source areas, should be recognizable in sediments in deep water at the mouths of the canyons. Such studies might conceivably establish the whole sequence of events that beach sediments undergo from their source areas, down streams to the sea, along shore to canyon heads, down these canyons to deep water and their final resting place. Estimates of the quantity of sediments moved down the canyons would be desirable, in that light would be cast on the important ratio of amount of sediment moved through the canyons compared to the amount of erosion necessary to cut them. If this ratio is high, it is easier to understand how the moving sediment can cut the canyons. In this regard note that the number of submarine canyons now heading near sea-level, and therefore presumably the most active, is actually surprisingly small. Even if large amounts of sediment are moved seaward by some process not yet recognized and only relatively small amounts are carried along shore to the heads of canyons, the small number of canyons along the California coast suggests that very large amounts of sediment relative to the volume of the canyons are moved through the canyons. Quantitative studies of these amounts, even if only approximate, would be valuable.

A sequence of events involving the sediment transportation from beaches to deep water some distance from shore provides us with a possible mechanism to explain the occurrence of sand in Recent deep-water sediments. Perhaps in some areas such occurrences may be associated with near-by steep slopes or submarine canyons.

Although California submarine canyons are studied here, similar generalizations may apply to canyons elsewhere. Submarine canyons along the eastern

seaboard of North America notch the continental shelf but do not now extend into the present shore line and therefore, according to the present theory, are now being eroded very slowly, if at all. During the stages of Pleistocene ice maxima, when the sea was about 300 feet lower than today, slopes steep enough for canyon formation presumably existed just offshore. With the inundation of the coastal plain since then, however, the necessary condition of a steep slope has been removed, so that canyon-cutting has been inhibited. Such a sequence of events is implied by cores recovered from these canyons which show coarse Wisconsin sediment overlain by finer sediment, often clay, of Recent age (Stetson, 1949, p. 32).

But why, Stetson asks (1949, p. 31), are there no submarine canyons in the western Gulf of Mexico? Could it be because shore slopes are too gentle, wave-action too weak, and sediment too dispersed before it reaches the relatively steep offshore slopes?

TIME OF SUBMARINE CANYON-CUTTING

Because many of the California submarine canyons head at the present shore line and apparently cut very young rocks, it is contended here that these canyons are now in the process of being eroded. The three conditions listed above must clearly apply to these. Other canyons, which do not head at present sea-level, may have originated when sea-level was about 300 feet lower than today, perhaps during the Wisconsin stage of the Pleistocene. These canyons may still receive sediment from shore processes, but not to the extent of those heading at present sea-level, otherwise they would have eroded headward. Still others, perhaps, have not been actively eroded since before the Wisconsin. On the whole, the California submarine canyons were probably eroded more effectively during the glacial stages of the Pleistocene. At such times wave-action along the California coast was no doubt more intense, resulting in more vigorous sediment transportation, brought about primarily by the more southerly position of storm tracks.

The canyons which head at present sea-level may not have great antiquity, because our three conditions are related closely to the present topography and configuration of the coast. That is, they certainly are no older than the topographic features which control their location. In some places, as Monterey Bay, for example, the presence of a large river and similar coastal configuration may have prevailed back into the Pliocene, but at others these controlling features may be much younger. Submarine canyons at localities like the former no doubt have a complicated history. If a canyon exists, though its origin and principal development may have come about under somewhat different physiographic conditions in the past, it continues to trap sediment and therefore perpetuates itself. Under favorable conditions this may mean that headward erosion will keep pace with changing physiography, including those changes resulting from subsidence or a rise in sea-level. In this regard it should be pointed out that submarine canyons refract waves in such a way as to bring about wave divergence and a marked weakening in longshore currents at their heads (Munk and Traylor, 1947, p. 6). Hence sediment carried to the canyon heads is likely to settle, in view of the reduction in current capacity.

Some of the submarine canyons must

be very young geologically, if our three conditions of origin are admitted as a premise, because the coastal topography where they are located has only recently assumed a shape to satisfy these conditions. Hueneme Submarine Canyon, for example, stands at the edge of a prograding alluvial plain built up by deposits brought to the sea by the Santa Clara River. It seems likely that the canyon is located at Point Hueneme because the bend in the shore line at the point reduces the capacity of the longshore currents. Before the formation of the point, which must be young geologically, this condition would not have existed. It is possible, however, that the steep slope and abundant sediment are mainly responsible for the canyons bordering Oxnard Plain on the south, including Hueneme Canyon, and that the local reduction in current capacity plays a relatively subordinate part.

If it is established that sediments move down submarine canyons, as the result of accumulation in their heads of debris brought from rivers by longshore currents, several implications come to mind. In the first place, such a chain of events, because it is going on now, no doubt has operated in the past at different places from time to time. No longer are the canyons unique to the Pleistocene epoch, although, in certain areas, only during the Pleistocene may conditions have been right for canyon formation. But why, we might ask, have the canyons gone unrecognized in the geologic record? No completely satisfactory answer to this question comes to mind, although a few comments are pertinent. In the first place, the canyons are erosional or "destructive" physiographic features, forms which seldom are fossilized. Only under the most unusual circumstances are mountains and canyons on land preserved, and then only temporarily. Because our record of events in the distant past comes from sediments in their eventual resting place in basins, seas, and perhaps occasionally oceanic areas, and not from the environs of the submarine canyons themselves, it is not likely that submarine canyons will commonly be preserved. Furthermore, our conditions for canyon formation impose limitations. Around the margins of many of the epeirogenic seas of the past the slopes were probably too gentle to permit canyon development. Perhaps only in orogenic areas or along continental margins, and especially where wave action was appreciable, should we look for manifestations of the canyons.

SUMMARY

A review of the geology and morphology of submarine canyons in the light of active shore processes along the California coast today suggests:

1. California submarine canyons have been eroded by one or more processes operating within the ocean.

2. These processes have cut canyon heads in the recent past and are probably in operation today.

3. Three conditions appear to be necessary for the California submarine canyons: (*a*) a source of sediment (canyons head at or near shore where sediment can be carried to them by longshore currents from the mouths of rivers); (*b*) a steep near-shore submarine slope (canyons appear to be located where a steep submarine slope reaches in close to shore); and (*c*) reduction in longshore current capacity. If the two preceding conditions are fulfilled, canyons are found where sediment tends to accumu-

late because shore currents lose transporting power.

4. Arguments are advanced in support of the theory that canyons are cut by sediment moving seaward downslope under gravity.

5. Several California submarine canyons are being eroded at the present time. Others were probably eroded more effectively during the Pleistocene (Wisconsin) 300-foot lower stand of sea-level. Still others may have originated earlier and evolved with changing conditions through time.

ACKNOWLEDGMENTS.—Appreciation is here extended to Daniel I. Axelrod, James Gilluly, John W. Handin, William C. Putnam, and Alfred O. Woodford for critical reading of the manuscript. Discussion with these persons, as well as with M. N. Bramlette, Cordell Durrell, David T. Griggs, and John C. Ludwick, has been very helpful. The writer is grateful to Betty Bruner Crowell for the drafting.

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COMPOSITE ORIGIN OF SUBMARINE CANYONS[1]

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ABSTRACT

The investigation of submarine canyons is still in its infancy, and complete reversal of ideas may come from further studies. However, the information which has become available in recent years offers an explanation which seems to eliminate the necessity of calling upon the inadequately supported hypotheses of submarine erosion or huge sea-level changes. The new explanation includes: subaerial excavation of portions of the canyons at various times during the past when the margins were elevated above the ocean level; building of deltas on the outer slopes during this excavation; submergence of the canyons, with some accompanying fill but with preservation of the canyons through slides and turbidity currents acting along the old canyon axes; reshaping of the old deltas, with some enlargement of shallow trenches characteristic of steep delta fronts; marine deposition on the old land surfaces into which the canyons were cut but concurrent maintenance of the canyons by slides, so that the canyon walls grew higher as the submergence continued; and, finally, the reshaping of the canyon heads by the Pleistocene sea-level changes of a few hundred feet.

This composite sequence is supported in part by the detailed soundings obtained recently off the San Diego area, where delta-like features have been found out beyond the rock-walled canyons. These supposed deltas are trenched by marginally rimmed valleys similar to those of the Swiss lakes. The hypothesis is also supported by new evidence showing the adequacy of submarine mass movements in keeping open the canyon heads. Finally, the evidence for large-scale submergence of continental margins and of oceanic islands is becoming continually more complete. On the other hand, the new reasons offered for supporting submarine origin of the canyons are shown to be based on misunderstanding of the character of the canyons. The advocates of submarine erosion continue their failure to observe what is actually taking place on steep aqueous slopes where sediment is available for the erosion they infer.

INTRODUCTION

In the last few years several hypotheses have been offered which oppose the idea that canyons of the sea floors are the product of subaerial erosion. Unlike some earlier opposition, based largely on misinformation, the articles by Kuenen (1947 and 1950) and the accompanying article by Crowell deserve serious consideration because they discuss actual, instead of imaginary, submarine canyons and because they point out serious discrepancies in the hypothesis of subaerial origin. Their attack, however, has been directed against the theory of huge glacially controlled lowerings of sea-level as the origin of the canyons.

The glacial-control hypothesis was developed (Shepard, 1936) because it appeared to offer less objections than a series of alternatives, but with the realization that there were many obstacles in the way of its acceptance. Objections have subsequently increased. For example, the boring into the reef at Funafuti Atoll appeared to constitute a partial confirmation of a world-wide sea-level change, but subsequent borings at Bikini and elsewhere (Ladd *et al.*, 1948) demonstrated that the submergence of coral reefs has been long continued and could not have been related to the melting of huge icecaps. Similarly, the shallow-water fossils of the deep seamounts of the Atlantic and Pacific have proved to be early Tertiary or Cretaceous in age.[2] As a result, the indications of shallow-water conditions no longer favor a great sea-level lowering in the Pleistocene. The cores in the Mediterranean

[1] Manuscript received April 30, 1951. Contribution from the Scripps Institution of Oceanography, new ser., no. 552.

[2] Reported in talks before the Geological Society of America by Maurice Ewing, Edwin L. Hamilton, and Roger Revelle.

obtained by Pettersson (Phleger, 1947) can be presumed to extend down into Pleistocene sediments; yet they give no evidence that a large lowering of sea-level changed the Mediterranean into a fresh-water lake. Furthermore, the surveys of Mediterranean submarine canyons show clearly that their lower limits extend well below the sill depths (Bourcart, 1950). These observations, combined with the difficulty of explaining the size of glaciers and the absence of indications of drastic climatic changes or of high salinity accompanying a great reduction in the size of the ocean, leave little support for the icecap explanation.

ALTERNATE HYPOTHESES

Two alternatives to the glacial-control hypothesis appear at the present stage of investigation: (1) that the canyons were caused by submarine processes and (2) that the canyons were cut by streams as the result of former emergences at various times during the past and have been preserved or reopened by submarine processes.

By far the most appealing hypothesis related to submarine processes is that of turbidity currents (also called "density" or "suspension" currents) combined with landslides. This idea was first suggested by Daly (1936) and later was more fully developed by Kuenen (1947, 1950) and is now given new impetus by Crowell. According to the latter, sediment is carried by longshore currents to a locality where the current slackens. If such a locality has a steep slope, it is suggested that the introduced material moves seaward, excavating the great canyons of the continental slope and shelf.

I proposed an alternative idea some years ago (Shepard, 1934), to the effect that the canyons were cut by river erosion at remote periods during the past and then submerged diastrophically, being kept open by submarine landslides. Because of the discovery of late Tertiary formations on the canyon walls, I later rejected this hypothesis. The present article revives this idea and brings it into line with the latest discoveries. It is now suggested that the continental margins have remained more or less fixed in geographic position at least throughout the Cenozoic and perhaps over a much longer period. If this can be proved, the following events are indicated:

1. The slopes during the course of a long period of submergence have been occasionally elevated, allowing the incising of subaerial canyons and the building of deltas outside.

2. Many of these canyons have either been kept open or been reopened from time to time by landslides combined with turbidity currents. Others have been filled by sedimentation, particularly on gentle slopes.

3. In many areas the shelves during subsidence have been built upward hundreds or thousands of feet above the old land surface into which the canyons were cut, but the sediment tending to fill the canyons has been carried outward by sliding and turbidity currents down the axes of the canyons (fig. 1). The remnants of some of this sediment can be found on the walls, where it has been locally cemented into rock formations.

4. During the period of canyon-cutting, deltas should have been formed on the slopes outside the canyon mouths; but these, having been long submerged, have slumped toward the deep basins outside, and the action of turbidity currents on these masses has given them valley-like indentations which form the

outward extensions of the subaerially cut submarine canyons.

5. The canyons which remained unfilled were extended headward during a glacial-stage lowering of several hundred feet, and, as a result, Pleistocene material is truncated by the inner branches of the canyons. Furthermore, the canyon heads, having been excavated in relatively recent times, have more tributaries than the older canyons, where many tributaries may have been filled.

6. Slumping has acted on the alluvial and marine deposits at the canyon heads, allowing the formation of shallow valleys in this recent material. In addition, slumping has widened the canyons as it does on land.

The only striking difference between the two hypotheses is that the first assumes that no subaerial excavated canyons existed on the ocean floor in the area where the present canyons are found and hence that they were entirely the product of marine processes; while the second assumes that canyons were cut from time to time in the past and have been maintained and perhaps enlarged

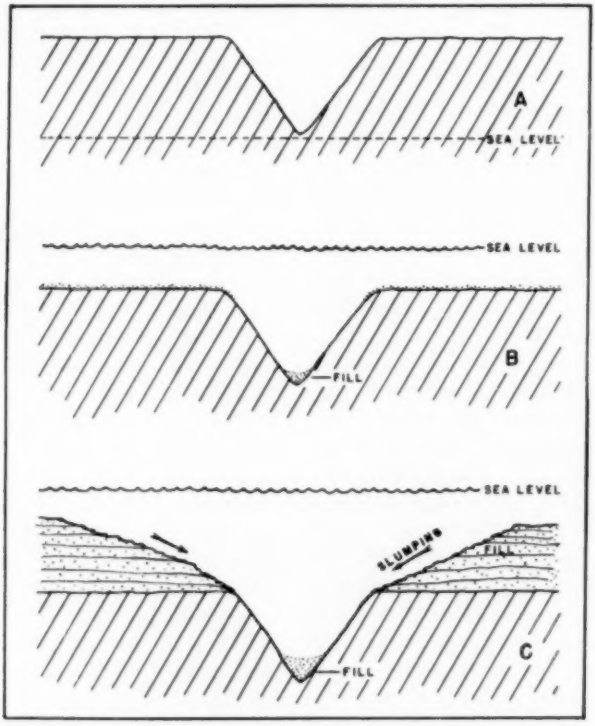

FIG. 1.—Illustration of a sequence which could account for greater canyon-wall height than elevation of continental margin above sea-level. *A* shows a section of the canyon cut into the margin during elevation. *B* indicates the submergence of the margin and initiation of deposition. *C* shows the building-up of the shelf accompanying submergence, while slumping and turbidity currents have prevented excessive fill on the floor of the steeply sloping submarine canyon.

by landslides and density currents. Both hypotheses seem to be dependent on the maintenance of the continental slopes, as it becomes quite obvious that an outbuilding shelf cannot have rock-walled canyons cut into it. If canyons had been cut into the slopes by subaerial erosion at any time during the past, they could, of course, be maintained by any process capable of cutting the canyons. Therefore, the important point to determine is whether or not the continental slopes have been exposed to subaerial processes.

EVIDENCE FOR SUBMERGENCE

Before discussing the possibility that most of the continental slopes of the world and even the slopes of some islands have been considerably submerged, it is important to consider the magnitude of the submergence necessary to account for the canyons. Echo soundings now reveal that submarine valleys can be traced outward to very great depths, even off the supposedly stable east coast of the United States. As indicated previously, however, the outer portions of the valleys may simply represent channels of nondeposition in deeply submerged delta fronts out beyond the lower limits of the subaerially excavated canyons. Therefore, the important point to determine is the depths to which the rock canyons or the deeply incised canyons extend. Unfortunately, we do not now have much evidence as to how deep below sea-level rock-walled canyons do extend. Rock has been dredged in the California canyons from depths of 3,900 feet (Shepard and Emery, 1941, p. 89) and from the Georges Bank canyons at 3,300 feet (Stetson, 1949), and rock was obtained in a core from one east-coast canyon farther south, at a depth of 5,100 feet (Stetson, 1949, p. 33). Survey reports suggest rock bottom in about 3,600 feet in Tokyo Canyon (Shepard, 1948, p. 219). The steep-walled gorges extend down as deep as 6,000 feet in some places, but for the most part the valleys become insignificant at greater depths.

Is it unlikely that portions of the continental slopes have formerly been as much as 6,000 feet above their present level? Support for such a contention is found in the records of well cores on land and from collections on submarine hills outside the coast. A drilling in Cape Hatteras shows the Cretaceous terrestrial formations at a depth of 6,500 feet below present sea-level (Swain, 1947). In Florida and around the Gulf Coast, oil-well drillings show that great submergence has taken place, accompanied by deposition of shallow-water formations. If the continental slope in these areas had persisted, canyons should have been cut during any emergence which alternated with the submergence. Such emergences seem to be indicated by the erosional unconformities of thick sedimentary series.

Off the coast of southern California we find many banks ranging in depths down to about 3,000 feet. These banks yield well-rounded cobbles, as do also the seamounts farther offshore. It is hard to explain the origin of these rounded rocks without shallow-water conditions during the past. Is it not likely that the shelves were also emergent at the time when the banks were at or above sea-level? The recent evidence of great submergence in the coral islands and on the flat-topped *guyots* of the Pacific is in line with the rest of the evidence.

Subsidence must have occurred in extensive areas in the world, and this same widespread subsidence could have brought the eroded portions of the submarine canyons to their great depths. It seems, therefore, rather pointless to attempt an explanation of the cutting of

the canyons without the help of the subaerial erosion which could have taken place during the earlier state of emergence.

INDICATIONS FROM DELTAS IN GLACIAL LAKES

One of Daly's principal arguments in favor of the origin of submarine canyons by turbidity currents is the existence of valleys on the fronts of deltas in the Swiss lakes. There is no doubt that such features exist (fig. 2), but they have only moderate depth, about 50–100 feet below surroundings. The gullies are also found on the front of marine deltas, such as the Fraser delta, where it is built into Georgia Strait (see Canadian chart 3429). According to several Swiss investigators (all quoted in Collet, 1925), these gullies are nondepositional features, resulting from deposition on either side like the natural levees of rivers in floodplains. The gullies occur in areas where abundant sediment is being carried down slopes which are, in many places, steeper than the slopes in which submarine canyons are found; and yet these delta slopes do not have canyons cut into them and, so far as we know, are actually prograding. Thus, under highly favorable conditions for underwater erosion, deposition rather than erosion is taking place.[3]

Considering the glacial lakes for further analogy, we find that on the mountainsides above the lakes various canyons have been cut since the end of the glacial period. These are often hundreds of feet deep. If erosion had been taking place on the adjacent steep-sloping lake floors at a rate comparable to that above the lake, there should be similar canyons below the water level. Furthermore, estuaries should be found at the mouths of the land canyons. Actually, there are deltas outside the postglacially carved valleys.

CONTRAST BETWEEN SUBMARINE CANYONS AND DELTA-FRONT DEPRESSIONS

It seems reasonable to suppose that the depressions found in the submarine portions of a delta are the result of combined landslide and turbidity-current action. Figure 1 shows the feature which was found on the steep front of the Rhone Delta in Lake Geneva. It will be seen that this valley has no tributaries, that it has little depth below surroundings, and that it has ridges or "natural levees" next to it. What greater contrast could there be between this valley and the inner rock gorges of Scripps and Tokyo canyons (fig. 3)? On the other hand, the outer end of La Jolla Canyon (fig. 4) is closely similar to the valley of the Rhone Delta.

It is not difficult to imagine the type of valley which would develop on a submarine escarpment, such as a typical continental slope, provided that there had been no subaerial erosion. The sediment on the slopes is capable of sliding, so that landslide scars should form at these places. They should originate at a variety of depths and should be somewhat discontinuous. If sedimentation did not intervene, they might remove all the loose sediment on the slope, down to bare rock. As a result, the sediment on the slope adjacent to the landslide scar would become unstable and tend to slide, moving in part into the neighboring depression. A series of slides might remove all or most of the slope sediment. In this way old rock surfaces could be

[3] Actual erosion caused by slides does take place at the upper part, leaving a shallow trench of short length and short duration (see Woodford, 1951, fig. 8).

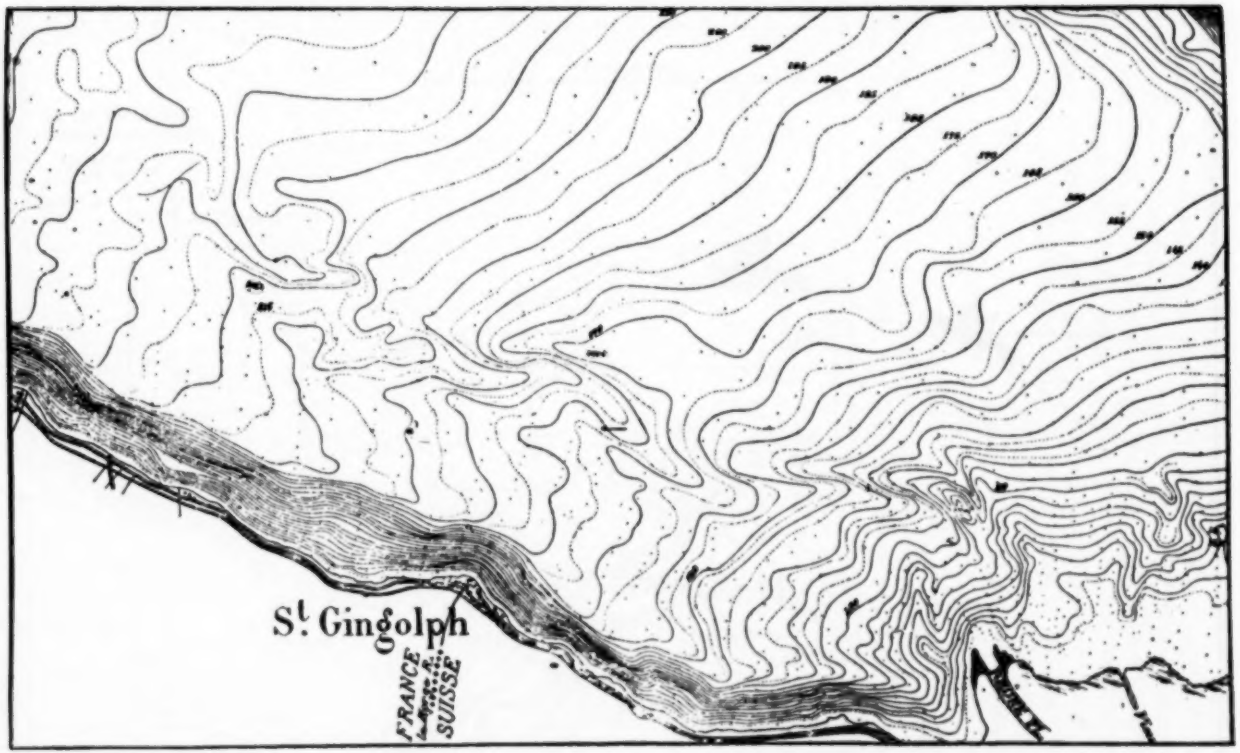

FIG. 2.—Gullies on the front of the Rhone Delta in Lake Geneva. Contour intervals: 5 and 10 meters. From Collet (1925, fig. 42). Compare fig. 4

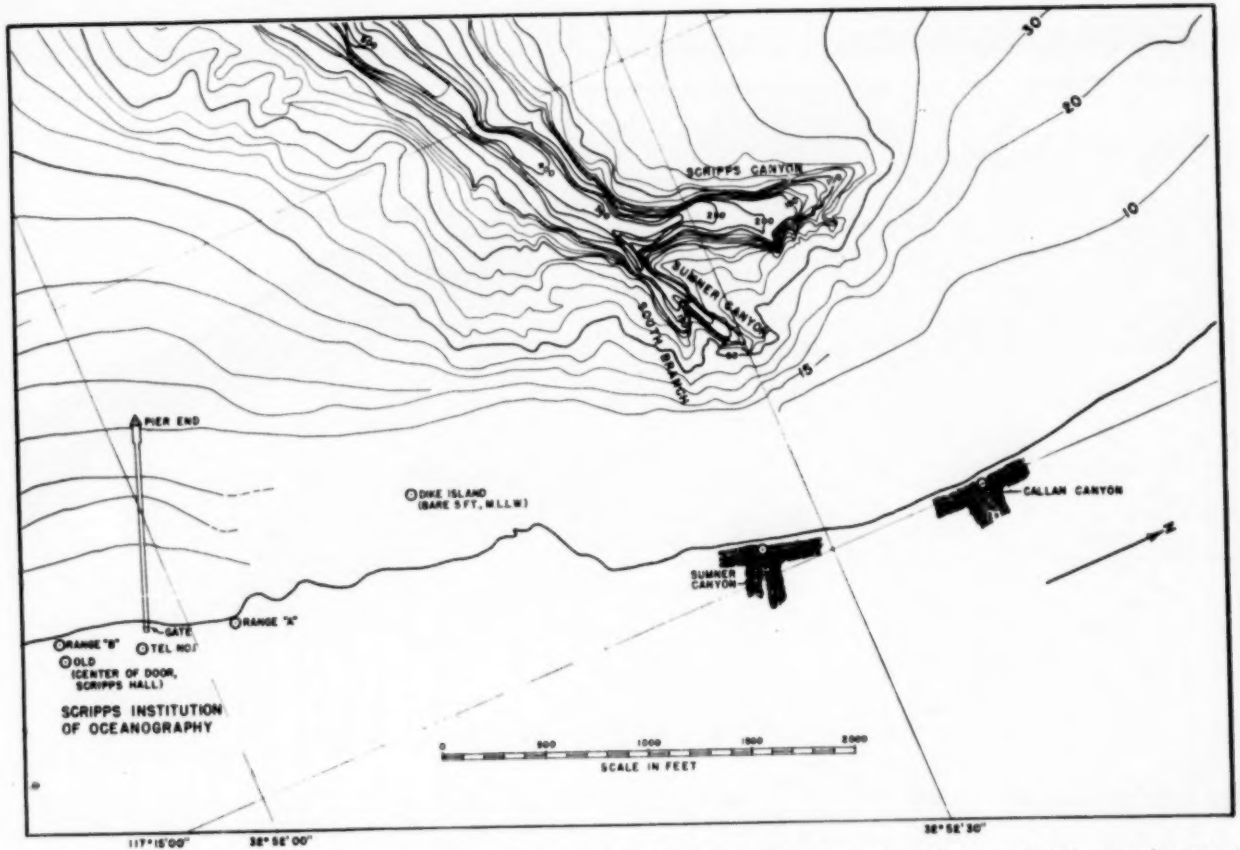

FIG. 34.—Scripps Canyon. Contour intervals: 5 feet to 20; 10 feet to 150; 50 feet beyond 150. Comparison with figs. 2 and 4 illustrates the contrast between this rock-walled gorge and the shallow valleys found in the submerged fronts of deltas.

resurrected from time to time, staying uncovered only while sedimentation was negligible; but, in the absence of an appreciable sediment supply, the cutting into the rock surface should be infinitesimally slow, particularly cutting into hard rock, such as the granite of Carmel or Monterey canyons.

LONGSHORE CURRENTS AND CANYON LOCATIONS

Crowell has discussed at length the significance of the supposed location of canyons at points where longshore currents are slowed down, causing deposition of sediments, which he considers would be available for the submarine erosion of the canyons. It is true that the currents are slowed down in the vicinity of canyons as the result of wave-convergence patterns (Shepard, 1950; Shepard and Inman, 1950). This effect, however, is the result of the presence of the canyon, and the retardation actually takes place updrift from the canyon head. In bays lacking canyons, the longshore currents

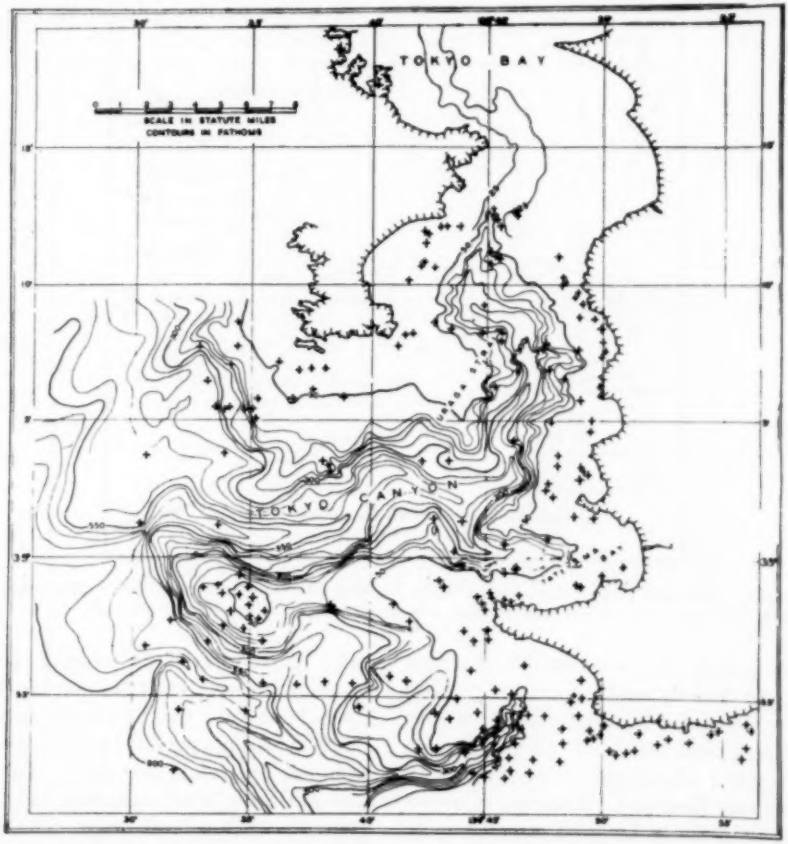

FIG. 3B.—Tokyo Canyon. Note that the rock bottom is indicated by crosses. Contour intervals: 50 fathoms, with the 25-fathom contour added in part. Comparison with figs. 2 and 4 illustrates the contrast between this rock-walled gorge and the shallow valleys found in the submerged fronts of deltas.

turn seaward next to points. Because the currents move predominantly from north to south in this region,[4] they are almost invariably deflected seaward at the north side of each projecting point. A careful examination of the California submarine canyon locations shows that there is only one good example of a canyon cut directly north of a point. Several others head within a half-mile of the projecting points, but not where currents normally turn seaward. For example, a northwest blow at La Jolla causes a current along Point La Jolla directly north of the point and well south of the canyon (fig. 5). Crowell recognizes this difficulty so far as Carmel Canyon is concerned but states that the sand necessary to cut this canyon has come around Point Pinos. Actually, the beaches are of such a nature in this area as to indicate that there is no appreciable transfer around these rocky promontories. Extensive beach sampling showed that the main beach at Carmel has sand that is similar to that found in various pocket beaches around Monterey Peninsula, but the sand at the head of Carmel Canyon is strikingly different and has a local origin. The canyons which enter deep estuaries like the Congo (Shepard, 1948, fig. 74) and To-

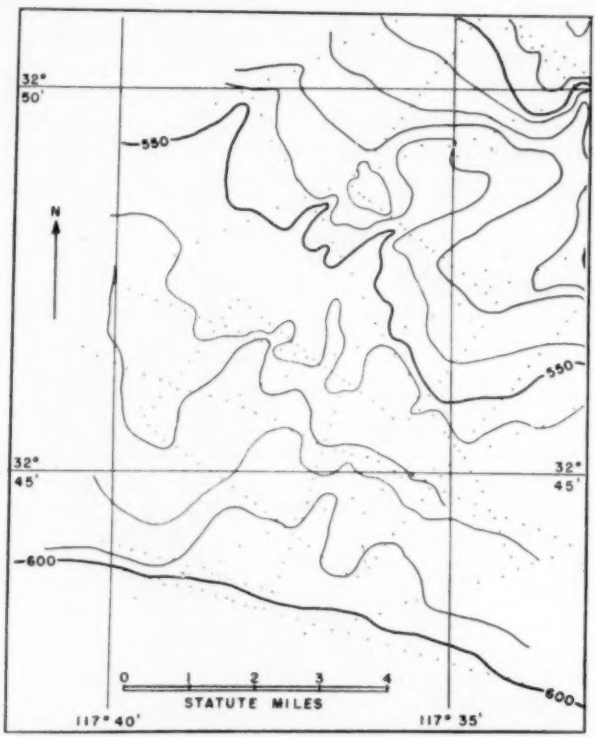

FIG. 4.—The outer portion of La Jolla Canyon. Contour interval: 10 fathoms. Dots indicate positions of soundings. Note resemblance to delta-front valleys and contrast to the inner rock gorges.

[4] Reference is to the California coast.

kyo Bay (fig. 3) clearly have no relation to longshore currents.

OBJECTIONS TO SUBAERIAL EROSION OF CANYONS

MAGNITUDE OF EMERGENCE

The most commonly cited objection to subaerial origin of canyons is that it is impossible that all the margins were elevated 10,000 feet in the Pleistocene. No such claim, however, is made, nor is it necessary. Actually, an uplift of about 1,000 feet could account for most of the canyons, provided that the upper portion of the canyon walls consisted of formations laid down after the canyons had been excavated. That is, the shelf margins could have had canyons 1,000 deep cut into them during some early stage and then could have been submerged, followed by the building-up of shelves of younger material and the slumping of portions of this material out along the axes of the canyons (fig. 1).

In view of what we know of the frequent periods of rejuvenation which occur on land surfaces, followed by peneplanation, it would be amazing if during

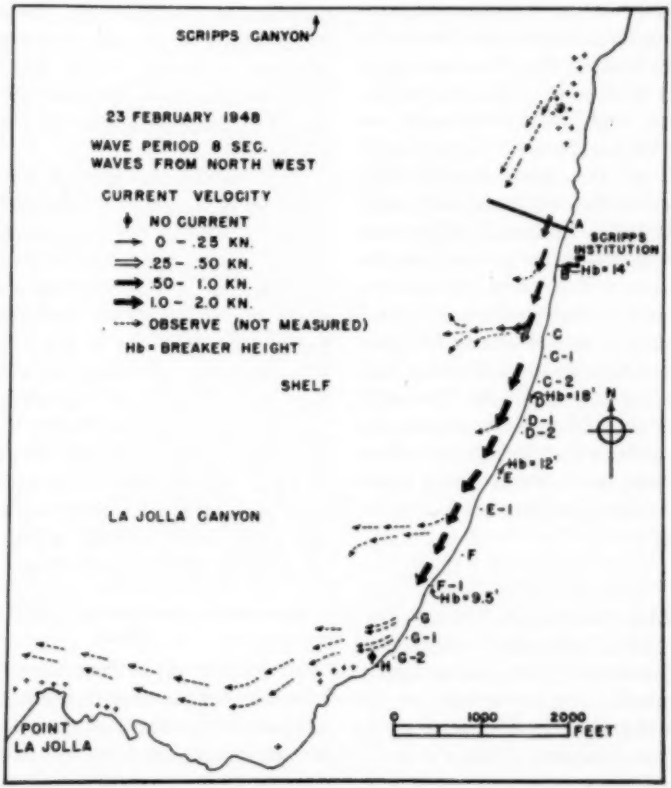

FIG. 5.—Longshore drift accompanying a northwest blow. Note that the principal outpouring of outmoving current hugs Point La Jolla and extends well south of La Jolla Canyon.

the long history of the shelves and continental slopes there had been no episode when moderate uplifts had taken place. On the other hand, some of the submarine canyons, such as Monterey, appear to require rather large-scale emergence of the margins. In view of the well-known instability of the earth's crust, geologists should not be disturbed by such a finding in a few localities.

SCARCITY OF CANYONS

Mention has been made by Crowell of the small number of submarine canyons in contrast to canyons cut into mountain slopes on land. Actually, as soundings increase, submarine canyons are constantly being discovered, so that the contrast in some cases is now very slight. On the other hand, real contrast should not prove disturbing because active sedimentation on the slope should have filled many of the canyons with sediments. Similarly, the small number of tributaries in the outer gorges may be more apparent than real. Submarine trenches, such as Bering Canyon (north of the Aleutians) and Monterey Canyon, have a considerable number of tributaries entering at great depth. No doubt others have been filled. Furthermore, the outermost portions of the marine valleys may lack tributaries because they are of submarine origin like the trenches of the Swiss lakes.

CANYON WALLS OF RECENT ALLUVIUM

Crowell has referred to the evidence that the head of Redondo Canyon is cut into Pleistocene and even Recent formations. Similarly, the formations on the walls of the gullies at the head of La Jolla Canyon (Shepard, 1949, pls. 7–11) are alluvial materials which are very likely not older than Pleistocene and may even be Recent. Crowell considers that these young formations constitute evidence that the canyons must be submarine in origin and still in the process of formation. The point deserves serious consideration.

Turning, first, to the question whether the canyons are still being formed, we are dealing largely with a definition. If the canyons have been cut subaerially and subsequently submerged, leaving steep slopes, there is no reason why the walls should not be subject to slump, or any reason why there should be complete cessation of headward growth, which is caused to a considerable extent by mass movements. Furthermore, the sediment which tends to fill the canyons is also affected by slump, which should leave steep remnants on the sides of the canyon. The same principle is illustrated in figure 1.

The Pleistocene age of the canyon heads is thoroughly understandable because, according to conservative estimates, the sea-level was lowered several hundred feet during the stages of maximum glaciation. This lowering would have allowed streams to flow into the unfilled heads of previously excavated valleys and to have cut the inner gorges, such as those explored in the vicinity of La Jolla (Shepard, 1949). The evidence of these Pleistocene formations, therefore, does not necessarily indicate that the great outer portions of the canyons were cut at such a recent time.

ABSENCE OF LAND VALLEYS INSIDE CANYON HEADS

A number of writers have been impressed with the discovery that there are canyons off portions of the coast where no streams or land valleys exist at the present time. Crowell considers the absence of any evidence that a valley ever existed inside Redondo Canyon as con-

clusive proof that the submarine canyon was not subaerially eroded. This objection is not so significant as it might seem if one considers other possible sequences of origin. In the first place, one could assume that the subaerially excavated valley never extended landward of this point. Valleys cut on slopes do not extend indefinitely into the plateaus at their heads. In the second place, the canyons may have been excavated before the late Pleistocene and Recent fill were deposited, having been maintained during this deposition by sliding and turbidity currents acting along the canyon axes.

SUMMARY AND CONCLUSIONS

It has been shown that the objections offered to the idea that submarine canyons are the product of subaerial erosion apply only to an abandoned hypothesis that the canyons were cut as the result of huge changes of sea-level during the glacial period. They do not apply to an alternative hypothesis that the canyons were eroded at various times during the past, when the margins happened to be above sea-level, and that they have been maintained subsequently by the sliding-out of the sediment fill and by turbidity currents. It has also been shown that the amount of vertical movement necessary to account for the canyons is not as much as the greatest valley depths indicated in canyon surveys, because the outer valleys are probably due to modification of submerged deltas. The height of walls of canyons is considered now to be a combined product of stream excavation and building-up of sediments on the old surfaces into which the canyons were cut, while slides kept the canyon axes from being filled. The supposed Pleistocene origin of the canyons is now explained as the last of a sequence of events and as the result of exposure of old canyon heads during Pleistocene sea-level lowering which caused headward growth of the old canyons.

The new hypothesis requires supporting evidence of widespread and long-continued subsidence of the continental margins and oceanic islands. Such evidence is becoming available from well records in many areas, including, among others, Cape Hatteras, the outer Mississippi Delta, and various coral atolls. Submergence is also indicated by the numerous finds of rounded cobbles and of shallow-water organisms on the flat-topped banks and seamounts of the Pacific and Atlantic. In view of all this independent evidence, there should be little difficulty in accepting the implication of submergence which has been one of the stumbling blocks in the path of the idea of the subaerial origin of the canyons. On the other hand, the advocates of submarine origin should no longer overlook the implications which are developed by the evidence of submergence.

It is perhaps unfortunate that the advocates of submarine erosion have carried on so little field work in the canyon investigation. The failure to appreciate conditions as they exist in the field is a decided handicap. Thus the suggestion that the California submarine canyons occur on the north side of points, where currents are deflected seaward, is based on deceptively small-scale charts and actually is almost entirely contrary to the facts which we have been gathering (Shepard and Inman, 1950). The steep fronts of deltas built into lakes and fiords appear to offer one of the best testing grounds for evaluating submarine erosion. Presently available evidence suggests that deposition is the dominant process in these places and that any valleys which result are due to localized in-

hibiting of the deposition either by slides or by turbidity currents. It is to be hoped that someone will investigate both the currents and the depth changes in these ideal localities, so as to see whether turbidity currents really do attain the high velocities which are now claimed for them without any observational basis. It would be a satisfaction to know whether sediments, upon being introduced to slopes of a few degrees, produce erosion or deposition.

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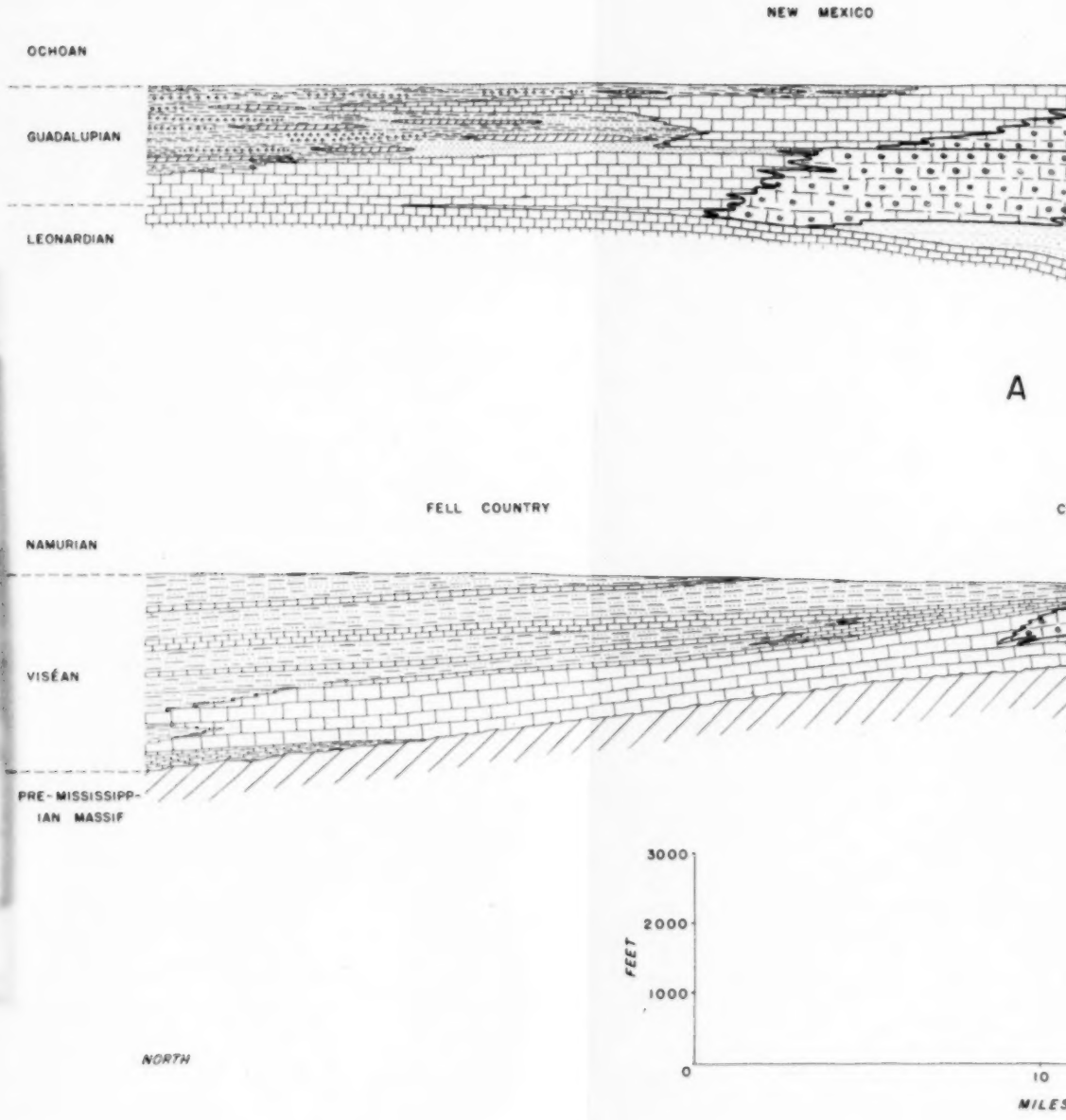
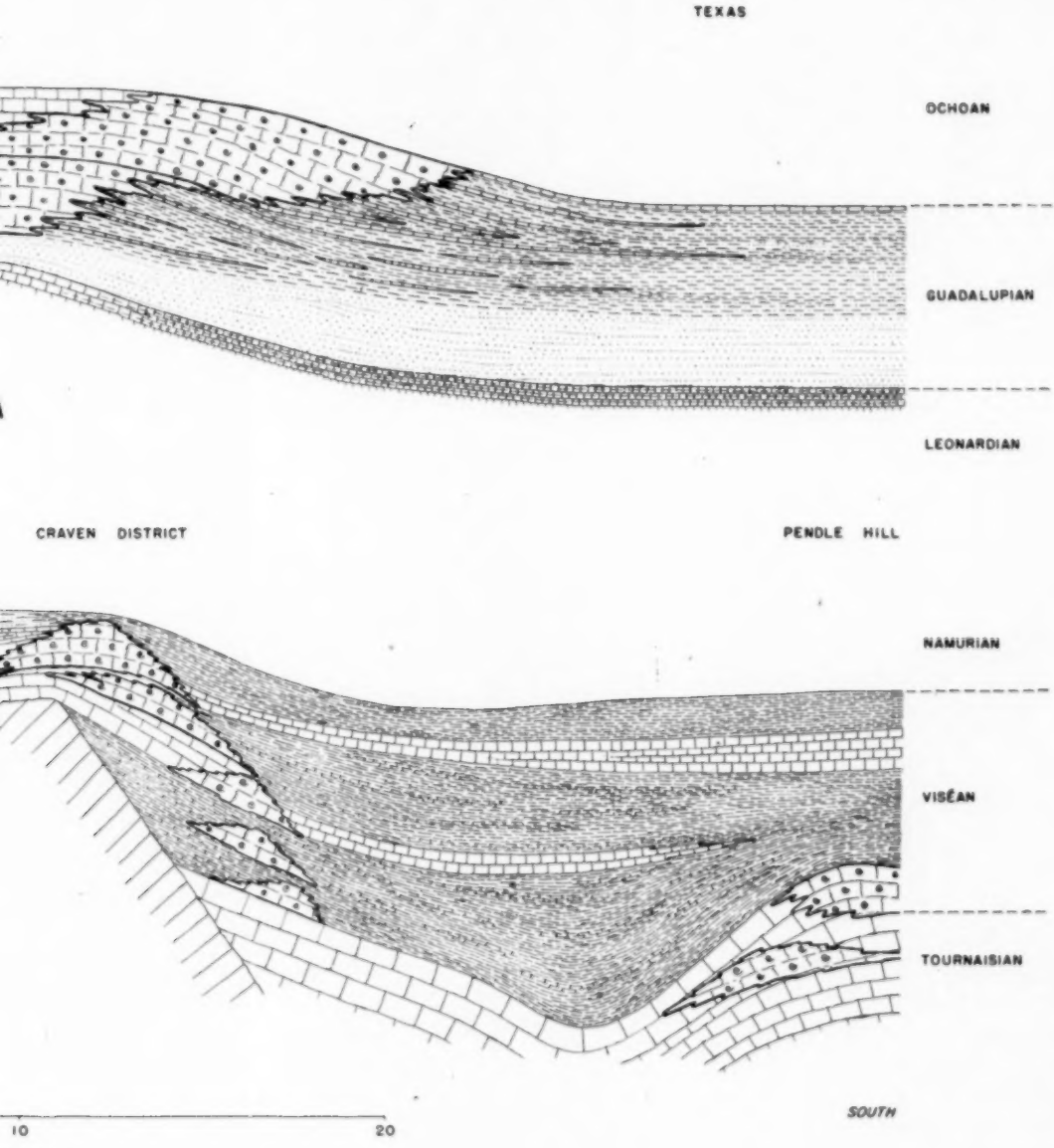


FIG. 1.—North-south sections to same horizontal and vertical scales of *A* Upper Permian (Guadalup
B Upper Mississippian (Viséan) reef suite of northern En



B

adalupian) reef suite of Delaware Basin, Texas and New Mexico (adapted from Lang, 1937), and
ern England (Pennines) (adapted from Wray, 1948)

GEOLOGICAL NOTES

NOTE ON MISSISSIPPIAN AND PERMIAN REEF SUITES[1]

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The July, 1950, number of the *Journal of Geology* is devoted to a number of papers on past and present reef structures. The first paper is an able and illuminating summary by Adams and Frenzel of the environment and sedimentation of the now famous Capitan Reef and associated suite of deposits of Upper Permian (Guadalupian) age in Texas and New Mexico. The second paper is an equally important, but less impressive, analysis of the Upper Mississippian(Viséan) reef suite of the Pennine region of northern England. The apparently fortuitous juxtaposition of these two papers suggests that a comparison of these two Paleozoic reef areas should be mutually useful to students of stratigraphy.

But even a casual survey of the extensive literature on the two areas (see King, 1948, containing many references; Bond, 1950, containing many references to British reef structures; Pugh, 1950, a very useful paper, although several significant British papers are omitted) shows that their interpretation has developed independently, with modern reefs, which are not entirely homologous, as the only common frame of reference. One is surprised to find no mention of similar Paleozoic reef-bordered basins in the definitive papers of P. B. King on the Permian area, and very little in the papers by our British friends on the Mississippian Basin of northern England (Parkinson, 1943) and it comes to mind that the work of each might have been eased by study of what the others had done and thought. The purpose of this note is not to make the suggested extended comparison but rather to point out the remarkable similarities of these two great structures, in the hope that others who are closer to these special stratigraphic-paleoecologic-sedimentologic problems may profit by and extend the work of one another.

The diagram (fig. 1) emphasizes the homology between the reef-bordered basins of the Mississippian of England and the Permian of Texas and New Mexico. North-south generalized sections of the critical part of each, to the same horizontal and vertical scales, indicate their similar magnitude both in time and in space. The Permian Delaware Basin, however, is larger areally, enclosing at least 10,000 square miles to the Mississippian Pennine ("Central Province") Basin's 4,000 square miles. Both are of structural origin, the Pennine Basin being dominated on the north by the Craven fault system and bordering rigid pre-Mississippian massifs, whereas the Delaware Basin was apparently controlled by local downward flexing without fracturing of earlier Permian rocks. In both cases, however, results were similar: deep euxinic (Pennine Basin) or para-euxinic (Delaware Basin) marine basins, more or less completely enclosed by steep slopes leading to shallow shelves extending toward more or less distant shore lines. Organic reefs grew in the optimum environment presented by the edges of the shelves bordering the basins, with the result that three main ecological and depositional situations, stressed by all workers on both areas, were established: (1) the lagoonal shelves (to the north in the particular sections, fig. 1), (2) the reef tracts (central in the sections), and (3) the deep basins (to the south in the sections), each with its char-

[1] Manuscript received July 25, 1951.

acteristic sedimentary and faunal phase.[2]

The shelves and basins were separated, and to a great extent their sediments were controlled, by the marginal reef tracts. The sediments and faunas of the reef phase are very similar in both basins, as expected because the environment in both cases was similar: massive, poorly bedded to unbedded complexes of various types of limestones, more or less lenticular in section, normal to the margin of the shelves, and more or less continuous as knolls or banks parallel to them. Fossils, other than algae and sponges, occur only in a few lenses; corals and bryozoans are uncommon.

The sediments on the shelves ("back-reef facies," "massif facies") behind the reefs and extending shoreward are not wholly alike in the two areas because of different climatic and geographic conditions. In both, however, "normal" fossiliferous limestones, pisolites, and chinastones extend back for some distance, passing into marine shales and sandstones (Yoredale group) in the Pennine Basin, and into dolomites, sandstones, shales, evaporites (Chalk Bluff group), and eventually continental beds in the Delaware Basin.

Basinward from the reef tracts the sediments are, at first, those characteristic of steep slopes: conglomerates, breccias, granular limestones, etc., becoming less massive and finer grained outward and passing or lensing into the basin-bottom deep-water deposits. In the Delaware Basin the latter are largely cyclic sequences of flat-lying fine sandstones and siltstones, with some bituminous limestones. The fauna is characterized by ammonites, but it is not clearly pelagic. The origin of these deep-basin clastics has not yet been satisfactorily explained. They are present in immense quantity, although somewhat thinner than the equivalent reef and shelf deposits, and apparently contain no shale or clay-sized particles except bentonites. If they represent material carried somehow from the shelf over the reef tracts, what became of the finest particles? The relationship of water composition, temperature, and circulation to sedimentary and organic environments has yet to be satisfactorily resolved. The studies now in progress by N. D. Newell and associates should be significant in this connection.

In the Pennine Mississippian Basin the deposits are more genuinely of the euxinic ("pontic") phase than in the Delaware Basin and are much thicker than their equivalent reef and shelf phase sediments. They consist of thin-bedded black bituminous shales and limestones, carrying a pelagic goniatite and thin-shelled pelecypod fauna. Their source is perhaps more obvious than that of the basin beds of the Delaware Basin: the by-passing of fine clastics across the incompletely enclosing marginal reef tract into a deep stygian basin.

[2] For the use of "phase" rather than the overworked term "facies" see R. S. Allan (1948, p. 8).

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DISCUSSION

ORIGIN OF MICRORELIEF IN THE LAKE AGASSIZ BASIN[1]

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Currently there is much interest in various so-called "periglacial" formations and phenomena. Among these formations are some peculiar forms of microrelief and various soil features, such as involutions, wedges, and polygonal patterns of soil complexes. There is a strong tendency to attribute these features to the perennial freezing of the ground, which presumably affected wide areas during the glacial ages. New terms, such as "cryopedology," "congeliturbate," "congelifraction," and "permafrost" have been coined, and strong emphasis is being placed on partly real and partly assumed similarities between these various features and those found in the presently frozen arctic tundras and adjacent subarctic regions.

In a recent very interesting and thought-provoking paper on presumed periglacial features in the Lake Agassiz Basin, Horberg (1951) describes some of these features and suggests an explanation of their origin. Some of the assumptions on which the interpretation of the field data are based, however, appear to be open to criticism.

The author describes intersecting minor ridges which form the microrelief of a certain part of the area formerly occupied by the glacial lake and states in the abstract of his paper that "it is proposed that the ridges represent frozen-ground structures formed during retreat of the late Wisconsin ice." This proposition is based on the assumption that, following the draining of Lake Agassiz in the closing "stages of Wisconsin time," a periglacial environment persisted long enough for perennially frozen ground to develop in the lake sediments and that the former existence of permafrost in the area is evidenced by deformed lake clays, involutions, wedge structures, and polygonal ground (pp. 16–17). The periglacial environment is described as being characterized by strong winds, low temperatures, permafrost, and intense frost-action (p. 9).

To begin with, some doubt may exist as to the use of the term "periglacial" (Lozinski, 1909). The term is somewhat vague. The prefix *peri-*, borrowed from the Greek, means essentially "around," "enclosing," or "surrounding" with reference to space rather than time. "Periglacial" refers to something taking place just beyond the border of the glaciated area contemporaneous with, and presumably caused by, glaciation of the neighboring region, but not something just preceding or immediately following the glaciation. Periglacial phenomena are confined to the zone "marginal to the Pleistocene ice sheets" (Smith, 1949, after Horberg). If this definition of the term is correct, then "periglacial" hardly would apply to anything that occurred in the area in question after the draining of the lake, which was accomplished well after the withdrawal of the glacier to a point a considerable distance beyond the border of the region.

Moreover, perennial freezing of the ground is not a periglacial phenomenon. Perhaps it could be regarded as a sort of "subterranean glaciation," characterized by chilling of the earth's crust and the formation of the ice inside the ground rather than on its surface. Such freezing of the ground takes place under a condition of decidedly negative mean temperature combined with low precipitation, especially low snowfall. This climate is not glacial. The latter is characterized by perhaps equally low temperatures and much heavier winter precipitation. The ground is perennially frozen throughout enormous areas in eastern Siberia, none of which, save the high mountains, was glaciated during the Pleistocene and is without any glaciation past or present of the adjacent regions. Hence this perennially frozen ground is not by any means a periglacial phenonenon. Neither is the perennial freezing of the tundras.

[1] Manuscript received May 3, 1951.

Solifluction, involutions, polygonal patterns of soil complexes, peculiar forms of microrelief, frost-cracking, ground-ice formation, and other similar phenomena are common throughout the regions affected by perennial ground frost, and thus none of them is necessarily a periglacial feature.

The big question, however, is whether it was possible for the sediments of Lake Agassiz to become perennially frozen subsequent to the draining of the lake.

The area described by Horberg is in the middle and deepest part of the depression which was occupied by Lake Agassiz. Obviously, this area was drained and became dry (land) at the very end of the life of this lake. Practically the entire area is underlain by several tens of feet of lacustrine, partly varved clays, in places interbedded with sheets of silt. Certainly, these sediments could not have been frozen before the area was vacated by the Wisconsin ice and its meltwater, because they did not exist at that time. Nor could the sediments freeze before the lake was drained, because they were protected from the rigorous climate by deep water. Thus Horberg suggests that the ground froze after the draining of the lake.

At this point one may indulge in a little calculation and some reasoning about the sequence of postglacial events. The data obtained by the radiocarbon technique indicate that the age of the Mankato drift is of the order of about ten to eleven thousand years (Arnold and Libby, 1951; Flint and Duvey, 1951). The formation of this drift antedated the beginning of Lake Agassiz, or, at most, it could have been "associated with the early history" of this lake.

The length of life of Lake Agassiz cannot be ascertained at present. It is fairly well established, however, that during one stage of its history this lake drained southward through the Minnesota River until a northward outlet became available. It is generally belived that the valley of the Minnesota River was eroded largely, if not entirely, during this time (the Traverse stage of Lake Agassiz as conceived by this writer, 1947, or the pre-McCauleyville stage of Upham, 1896), with little additional erosion after the diversion of meltwaters northward. The size and depth of this valley may be taken as a measure of the duration of the Traverse stage of the glacial lake. No precise estimates of the length of this period have been attempted, but it seems reasonable to assume that the development of this valley should require several thousands of years. The other criterion for judging the length of this stage is the undercutting by the lake of a steep escarpment along the strand line marked by the magnificent Campbell beaches.

It follows that Lake Agassiz ceased to discharge its water southward not much more, if not less, than, say, six or seven thousand years ago. At that time water stood about 970 feet above sea-level, and the depth of the lake near the present international boundary was of the order of 100–150 feet. The margin of the glacier retaining the lake probably was some 100 miles above the forty-ninth parallel.

The last stage (Kent) of Lake Agassiz probably was considerably shorter than the preceding stage because lowering of the lake's level during this stage was not hampered by the necessity of deepening the channel of the outlet, as in Brown's Valley in the Big Stone Lake region. Hence, perhaps, this stage did not last much longer than a thousand years. It follows that the last water of Lake Agassiz could have been drained from the area south of Lat. 49° N. not much more, if not less, than, say, five thousand years ago. In fact, the time which elapsed after the draining of Lake Agassiz might have been much shorter than five thousand years. A conspicuous freshness of the features left in the area by the extinct lake, including beaches, shore lines, remnants of aquatic plants in laminated sediments, meager oxidation, and ineffective leaching of lacustrine sediments and even the microrelief which probably was created by wave-action rather than by problematic postlake frost-action—all seem to indicate an extreme geological youthfulness for the landscape.

Now what are the probabilities of the freezing of these sediments after the draining of the lake?

Lake Agassiz was formed by ponding of meltwater. Wisconsin ice was steadily melting. Hence the climate was no longer "glacial." Deglaciation may take place, indeed, without a rise of mean temperature, provided that there is a sufficient decrease in winter precipitation. Presumably, that is what is going on now on Novaya Zemlia, Severnaya Zemlia, and some other arctic islands. In such cases, however, shrinkage of the ice is due largely to sublimation. Then there is very little meltwater. If such had been the case with the ablation of the Wisconsin ice, then perhaps there would have been no Lake Agassiz and no Minnesota River Valley.

Extinction of Lake Agassiz was accompanied by extensive formation of peat bogs, which filled its shallow lagoons throughout Minnesota. This, again, indicates that xeromorphic conditions, under which the Wisconsin ice would shrink without the climate's getting warmer, are rather improbable, although a short and more recent relatively dry and warm climatic substage is indicated by pollen analyses.

The border of the remnants of Wisconsin glacier should have been at least 150 miles and perhaps considerably farther from the area when Lake Agassiz withdrew from it. Hence the remnants of the glacier would hardly have affected too strongly the climate of the deglaciated region. Thus it appears that conditions necessary for perennial freezing of the ground did not exist in the area during any part of the late Wisconsin and post-Wisconsin interval.

The field data which are presented by Horberg as evidence for the past perennial freezing of the lake sediments are far too meager and uncertain as to their origin to be convincing. An external and, in fact, not too close analogy between these various so-called "periglacial" features in the Lake Agassiz Basin and those found in regions which are now affected by perennial ground frost is, indeed, not a proof of a similar origin of such features.

Some field data presented are hardly in line with Horberg's reasoning. For example, involutions are illustrated by an exposure at a locality which is at the edge of a moraine and, perhaps, shows the arrangement of morainic material rather than deformation of the lake sediments. The involuted zone is at a depth of 16.5 feet, overlain by a 15-foot layer of clay and silt and 1½ feet of recent soil. Beneath these there is another "involuted" layer at a depth of 20–21.5 feet. Now involutions due to the permanent freezing of the ground do not and cannot take place at such a depth. They develop at the base of an "active layer," i.e., the uppermost layer of the ground, which is subject to alternating seasonal thaw and freeze. The maximum thickness of an active layer is less than 10 feet. In fact, it hardly exceeds 6 or 7 feet anywhere. Involution requires a fairly high, frost-generated hydrostatic pressure, which cannot develop below a depth exceeding a very few feet, say, 3–5. Therefore, if the layer at a depth of 16 or 20 feet is really involuted, it would indicate that, subsequent to formation, it had been covered by more recent sediments, the thickness of which was at least 10 and probably more than 15 feet. This, in turn, would indicate that the lake was still in existence and that freezing of the ground and involutions would take place under water, which, obviously, is impossible. It might be, of course, that the clay and silt above the involuted layers are not lake sediments but a recent overwash from the neighboring morainic hills. If such were the case, however, it certainly would have been noticed by Horberg.

The general wedges are illustrated by an exposure at the margin of the Pembina Delta. Here, again, the structure may or may not show deformation of the lake sediments subsequent to their deposition. Deposition of lacustrine sediments at the margin of the delta could alternate with periodic influxes of deltaic drifts. The deltaic alluvial fan could have been subject to dissection by runnels, and these could have been choked by more recent gravelly drifts. Marginal parts of the delta, naturally, were built under water. Hence interbedding of the Pembina alluvial drifts with lake sediments is to be expected and can be recognized on the profile described by Horberg in his table 5.

Soil wedges hardly need mentioning in this connection. They were formed and still are forming. The mere fact that they are marked by black material similar to that of the upper soil horizon indicates that this feature is related to the existing soils. Formation of such soils under the tundra, let alone periglacial environments, however, is entirely out of the question. Therefore, if cracking of the ground was due to frost-action and ice wedges, these cracks would have been filled by other materials long before the origin of the present black soils, and the inherited "wedges" would not be black. This is, indeed, recognized by Horberg in the statement on page 14 that "the periglacial features and the minor ridges formed during the same interval, following the draining of the lake and *preceding the development of the present soils* . . ." (italics mine).

And, finally, a few words about the minor ridges may be in order. Horberg considers two alternative interpretations of their origin: (1) ridges could have been formed by "fracture fillings deposited between blocks of ice at the closing stage of Lake Agassiz history," and (2) they might represent "tundra ridges due to ground-ice wedges."

The first hypothesis hardly needs to be considered. As Horberg points out, it "breaks down by its failure to explain the fact that . . . the ridges . . . are younger than the shore lines"

(p. 15). Whether ridges are younger than the shore lines or not, the mere suppositions on which this hypothesis is based seem entirely untenable. The hypothesis presupposes that the lake in its closing stage would freeze to the bottom, that the bottom ice would break into enormous blocks, and that the fractures in the ice would be enlarged by water and ultimately filled by sediments, while the intervening ice blocks would remain in the same position until they melted. To expect all these things to happen is, perhaps, to request too much from nature.

With certain reservations, Horberg favors the second hypothesis. He states that "except for the remarkable fracture pattern, there would be little hesitancy in interpreting the ridges as tundra forms. The fracture system thus suggests that more than one process may have been operative and that pre-existing structures of the lake floor may have determined the pattern of the ice wedges." Thus, although the pattern of the present microrelief could have been determined by the other factors, the formation of ridges itself was due to the ice wedges.

Ridges in the polygonal tundra are presumably formed by the bulging of the ground because of the expanding ice wedges in frost cracks. These ridges are very low and just a few feet wide, whereas the ridges described by Horberg are, on the average, about 150 feet wide and range from 75 to 500 feet in width and from 3 to 10 feet in elevation. Now just what size of ice wedges should be driven into the ground to form such ridges? Again, where precisely should these wedges be driven into the ground—along the crests of ridges or in what is now the "swales"? In polygonal tundras ice wedges develop in furrows between the parallel ridges, and the latter encircle the depressed middle parts of polygons (Leffingwell, 1919). Horberg states that "in the Lake Agassiz area the critical evidence of single large wedge structures in the center of ridges or furrows along ridge crests was not observed. Whether this was because of their absence or because of the lack of suitable exposures is uncertain" (p. 16). If such is the case, then what is the foundation of the hypothesis? Let alone the absence of evidence of deformation of bedded sediments by the postulated ice wedges, a mere glance at photograph *A* in plate 3 accompanying Horberg's paper and an attempt to locate the position of such a wedge are sufficient to cast a very strong doubt as to the validity of this hypothesis.

Microrelief of one kind or another is a common feature of practically every landscape, although only the most conspicuous forms of it are generally recognized. A very gently undulating microrelief (swells and swales), essentially similar to that shown by Horberg in plate 3, *A*, is one of the commonest types of microrelief found throughout the broad plains, as well as on alluvial terraces. It may be perfectly true that similar types of microrelief might form in different ways under correspondingly different conditions. It seems, however, that wind and water, the latter either running or oscillating, are the most likely factors of such development. In this particular area the possible, if not inevitable, effects of wave-action should be considered first, and only if these effects are proved inadequate should some other factors to be looked for.

The configuration of the bottom of Lake Agassiz, especially during its latest stages, was such that practically all along its flat and low shores shallow water extended out for several miles. During the closing stage of the lake, as Horberg states, "only about 15 feet of water in the original basin would have been needed to cover the entire ridged area." With such depth, strong wave-action hardly would fail to affect the muddy bottom and produce some kind of microrelief. The present microrelief of the area may or may not represent the unchanged effect of wave-action. The original microrelief could have been modified by winds and running water after the draining of the lake and before its dry bottom was occupied by sod-forming vegetation.

The wave-action of Lake Agassiz was strong enough to build conspicuous gravelly beaches. Is it necessary that the work of waves should be restricted to the building of beaches, leaving the bottom between the beaches perfectly smooth?

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REVIEWS

The Petrology of the Igneous Rocks. By F. H. HATCH, A. K. WELLS, and M. K. WELLS. 10th ed. London: Thomas Murby & Co., 1949. Pp. 469; figs. 151. $4.00.

This is a new and considerably rearranged edition of the late Frederick H. Hatch's well-known textbook of petrology, which for the last fifteen years has been edited and brought up-to-date by Messrs. Wells. The book does not go deeply into theoretical petrology and petrogenesis but should be very useful to petrography students. In conjunction with other books, it can be recommended as a text for courses in petrology and petrography.

The new edition begins with a discussion of the chief rock-forming minerals of igneous rocks (pp. 29–137). The second part deals with the mode of occurrence and the internal structures of igneous rocks (pp. 141–178) and includes a brief chapter on the consolidation of magmas and deuteric phenomena. Perhaps this chapter could advantageously be enlarged and joined more closely with the following part on the "Systematic Description of the Igneous Rocks" (pp. 181–359). The new edition departs from earlier ones in grouping plutonic and volcanic rocks of similar chemical composition together instead of treating these two groups separately, and the degree of crystallinity is used as the basis of classification within each group. It is understandable that, occasionally, types and associations of rocks are emphasized that are prominently developed in the British Isles. The reviewer regrets that the chapter on pyroclastic rocks, which concludes this part, is a brief synopsis of three pages. The remarkable variability in texture, structure, and chemical composition of pyroclastic rocks, along with their wide distribution, would call for a more detailed description than is offered in this book.

The chapter on metamorphism of igneous rocks of earlier editions has been deleted. Instead, the authors have written a new one on "Petrogenetic Considerations" (pp. 363–382), in which the theories of differentiation and effects of assimilation are discussed. They have added a brief chapter on the "Origin of Granitic Rocks and Granitization" (pp. 376–382), written with commendable detachment and impartiality.

The last part of the book is, as in earlier editions, a review of the igneous rocks in the British Isles, grouped according to decreasing geologic age, well documented, and accompanied by numerous maps. One can hardly read it without wishing that all textbooks of petrology had a comparable chapter on the igneous rocks of the country in which they are published.

The typography of the book is very good; typographical errors are few and unimportant. Although in this edition photographs are not reproduced so well as in earlier ones, the book has gained considerably through the many excellent line drawings by M. K. Wells. Particularly, his drawings of thin-sections should be of much greater aid to the student in identifying minerals and textures than a comparable number of photographs, and his skilful reproductions of the geometry of crystal lattices and optics are likewise most welcome.

ROBERT BALK

University of Chicago

Exploration Geophysics. By J. J. JAKOSKY. 2d ed. Los Angeles: Trija Publishing Co., 1950. Pp. xvi+1195; figs. 707. $12.00.

A review of a new edition of a book in a field where there are several texts should consider the new edition from three points of view. First, how does it compare with the earlier editions? Second, how does it compare with the other books in the field? Third, what is the intrinsic value of the book?

A new edition affords the author the possibility of making additions, revisions and deletions. A glance at the new edition of Jakosky shows that he has taken full benefit of the opportunity to make additions. The new edition has 1,195 pages, whereas the first edition had 786 pages. There have been many revisions which, almost without exception, have improved either the accuracy or the readability of the discussions. Unfortunately, the author and his con-

tributors and collaborators have taken almost no advantage of the opportunity to make deletions. A number of the chapters contain substantial amounts of material that do not merit the space devoted to them.

The theoretical sections of a number of chapters have been rewritten. These include portions of chapters iii, iv, vii, viij, and xi, covering magnetic, gravitational, seismic and chemical prospecting, and bore-hole measurements. Revisions in the sections covering instruments and techniques are found in discussions of offshore prospecting methods, the air-borne magnetometer, secondary refraction seismic work, air-blast shooting, radio-location methods, the dipmeter, and caliper well logs. Chapter x, on "Radioactivity Methods," is entirely new. Chapter viii, on "Chemical Methods," is almost entirely rewritten as is much of chapter xi, on "Bore Hole Investigations." Only incidental changes have been made in the other chapters.

In 1940, three textbooks on exploration geophysics appeared in English. None has appeared since, although two are scheduled for publication soon. Neither Nettleton nor Heiland has issued a revised edition. Nettleton differs so greatly from Heiland and Jakosky in intent and scope that there is little point in comparing it with the other two. It remains the outstanding short geophysical exploration text. Heiland and Jakosky are similar, in that each attempts to cover the field of exploration geophysics completely and in detail. Each has its merits and its defects. Heiland does a more nearly complete job of reporting the material in the geophysical literature but has the defect of presenting much of this material in a completely uncritical manner. This is perhaps a necessary consequence of authorship by a single author in a broad field. Jakosky has the advantage of presenting material by a number of specialists, each of whom is well qualified to discuss his particular phase of the field. This has the literary disadvantage of complete lack of unity. It has the technical disadvantage of illogical assignment of space to various topics. This method results in some duplications, contradictions, and omissions.

In spite of its shortcomings, which in places are glaring, Jakosky's *Exploration Geophysics* is a very worth-while book for anyone concerned with the details of exploration geophysics. The viewpoint is generally quite practical, although seldom sufficiently critical. Its extensive list of patents is a valuable feature. This second edition represents a sufficient improvement over the first edition to justify its publication.

W. M. Rust, Jr.

Humble Oil and Refining Co.
Houston, Texas

The Nininger Collection of Meteorites: A Catalog and a History. By H. H. Nininger and Addie D. Nininger. ("Am. Metorite Mus. Pub.") Northfield, Minn.: Northfield News, Inc., 1950. Pp. 144; figs. 6; pls. 38.

This book is primarily a catalogue of the meteorites in the Nininger collection, complete with data on locality, date of fall or discovery, general composition, weights of total specimen and of sample still in the collection, and notes on the field incidents connected with the discoveries. In an eighteen-page Introduction the authors include a brief account of the history of the collection, the public lectures which led to many of the discoveries, and the problems of financing the program, as well as some discussion of individual specimens. There are six state maps, showing sites of discovery of the meteorites, and thirty-eight plates of etched sections and entire specimens.

Jean G. Simmons

University of Chicago

"Report on Rocks from West Antarctica and the Scotia Arc." By G. W. Tyrell. ("Discovery Repts.," vol. 23, pp. 37–102; figs. 13; tables 8.) 1945.

The southern end of Tierra del Fuego veers sharply to the east and appears to be connected with the Antarctic continent by means of an arcuate submarine ridge system, the "Scotia Arc," convex to the east. On the southeastern portion of the loop rise the South Orkney Islands; closer to the Antarctic are the South Shetland Islands, almost merging with Graham Land, which projects from the polar area toward the New World. Far off to the east, perhaps offset somewhat from the main ridge, are the South Sandwich Islands. Between these three groups of islands lie scattered stacks and small isolated islands.

The present report deals with rock collections obtained from this area during two trips, in 1934

and 1937, of the British ship, "Discovery II," and is to some extent supplementary to earlier reports on this region. Previous collections had suggested that the South Shetland Islands are composed mainly of ?Mesozoic sedimentary rocks. However, the new collections have yielded various volcanic rocks, ranging from dacite to rhyolite, with associated pyroclastics. Recent volcanic centers on the islands erupt for the most part andesitic and basaltic material and are of normal circum-Pacific composition. The sedimentary rocks are mainly a greenstone-graywacke series, and rocks of seemingly the same kind have been obtained from Elephant and Clarence Islands, about 200 miles northeast of the South Shetland Islands. It is significant that very similar suites of rocks have been described from Tierra del Fuego.

The South Sandwich Islands, far to the east, have yielded material for new analyses of the lavas of several active and near-active volcanic centers. Basalts and andesitic rocks from these islands seem to be more closely related to lavas from the West Indies and Antillean arc than to any Andean types; and Tyrell assumes that the South Sandwich Islands stand on a fissure system of the South Atlantic magmatic area. Hence the poleward continuation of the Andean system must lie farther west. The problem of where the Andean and Atlantic provinces may join was discussed by H. Reiter and Eduard Suess at the start of the century, and the new material and analyses permit a closer formulation of this interesting problem.

R. BALK

University of Chicago

Krystallometrisches Praktikum. By ROBERT SCHROEDER. Berlin: Springer-Verlag, 1950. Pp. viii+109; figs. 156. DM. 15.60.

This work consists of two nearly equal parts. The first part deals with morphologic principles and consists of five chapters devoted to symmetry, systems, and classes; axes and elements; face symbols; twins (very brief); and complication (30 pages!). The last part covers morphologic practice by the Goldschmidt technique. There are six chapters which cover projections, two-circle goniometry, graphical calculations, mathematical calculations, crystal drawing, and crystal model sets. The author cites thirty-five references, of which ten were published prior to 1850, twenty between 1886 and 1908, one in 1921, and only five after 1930 (except for Palache, Berman, and Frondel, 1944, the latest is 1938). The Index has less than two hundred items.

The first half of the book duplicates what appears in most texts dealing with morphology, except that the treatment is historical and quite detailed. The discussion of complication is outmoded by the generalized Bravais principle (see the last chapter of Phillips, 1946). The last half covers much the same ground as Goldschmidt's *Kursus der Kristallometrie* (1934). (This work is not even cited in the list of references.) But, whereas Goldschmidt presents the systems by means of a specific crystal illustrating each one, Schroeder handles them as noted above.

The technique given in chapter xi of using the gnomonogram to prepare nets for making hollow models of crystals, stated on page 191 to be new and undescribed, was published by the reviewer ten years ago. Figures 153 and 156 have several errors in labeling.

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D. JEROME FISHER

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ERRATUM

In "The System Al_2O_3-H_2O," by Guy Ervin, Jr., and E. F. Osborn, vol. 59, no. 4, p. 385, l. 14 of the second column, *read* $\beta = 1.659$ *instead of* $\beta = 1.649$.

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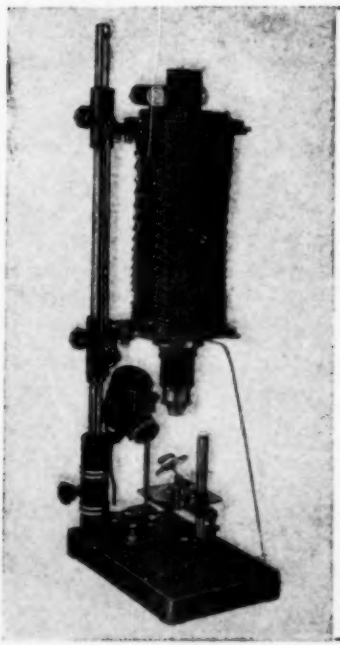

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